Vol. IX. MARCH, 1893. No. 11.

THE AMERICAN METEOROLOGICAL JOURNAL.

A MONTHLY REVIEW OF METEOROLOGY.

TABLE OF CONTENTS.

BOSTON, NEW YORK, CHICAGO, AND LONDON.

GINN & COMPANY,

Publication Office, 7-13 Tremont Place, Boston, Mass., U.S.A.

SINGLE COPIES . . 30 cents. | *PER ANNUM $3.00*

Entered at the Post-office, Boston, as second-class mail matter.

THE AMERICAN

METEOROLOGICAL JOURNAL.

Vol. IX. BOSTON, MASS., MARCH, 1893. No. 11.

EXPLORATION OF THE FREE AIR.*

PROF. MARK W. HARRINGTON.

IN an endeavor to revive American interest in the exploration of the free air there is especial propriety in presenting a paper on this subject in Boston, because it is the birthplace of the first man to make scientific aerial voyages.† This was Dr. John Jeffries, who made his ascents in England in 1784, and took with him not only a thermometer and barometer, but also a hygrometer and electrometer. In his second voyage he crossed the channel in a balloon, a feat which has hardly been surpassed since.

On the afternoon of June 17, 1843, the daring but sensational aeronaut, Mr. John Wise, made an ascent from Carlisle, Penn. He was very desirous of gratifying in some special way the great body of spectators. Seeing at some distance beyond and above him a huge black cloud, he determined, on the spur of the moment, to pass through it. His experience with this cloud proved to be much more exciting than he had anticipated, though the drama was played out of sight of the spectators.

Here is his account: ‡ —

"The cloud, to the best of my judgment, covered an area of from four to six miles in diameter; it appeared of a circular form, as I entered it, considerably depressed in its lower surface, presenting a great concavity toward the earth, with its lower edges very ragged and falling downward with an agitated motion, and it was of a dark smoke color. . . ."

* Read before a joint meeting of the New England Meteorological Society and the New England Association for Applied Meteorology, held in Boston Jan. 27, 1893.

† Am. Met. Journal, June, 1892; pp. 58-63. Also, Nov., 307-311.

‡ Through the Air, 1873; pp. 371-373.

Further on he says: —

". . . The cold had now become intense and everything around me of a fibrous nature became thickly covered with hoar-frost, . . . and the cords running up from my car looking like glass rods, these being glazed with ice and snow; and hail was indiscriminately pelting all around me. The cloud at this point, which I presumed to be about the midst of it, from the terrible ebullition going on, had not that black appearance I observed on entering it, but was of a light, milky color, and so dense just at this time that I could hardly see the balloon, which was sixteen feet above the car. From the intensity of the cold in this cloud I supposed that the gas would rapidly condense, and the balloon consequently descend and take me out of it. In this, however, I was doomed to disappointment, for I soon found myself whirling upward with a fearful rapidity, the balloon gyrating and the car describing a large circle in the cloud. A noise resembling the rushing of a thousand milldams, intermingled with a dismal moaning sound of wind, surrounded me in this terrible flight. Whether this noise was occasioned by the hail and snow which were so fearfully pelting the balloon I am unable to tell, as the moaning sound must evidently have had another source. I was in hope, when being hurled rapidly upward, that I should escape from the top of the cloud; but as in the former expectations of an opposite release from this terrible place disappointment was again my lot, and the congenial sunshine, invariably above, which had already been anticipated by its faint glimmer through the top of the cloud, soon vanished with a violent downward surge of the balloon, as it appeared to me, of some hundred feet. The balloon subsided only to be hurled upward again, when, having attained its maximum, it would again sink down with a swinging and fearful velocity, to be carried up again and let fall. This happened eight or ten times, all the time the storm raging with unabated fury, while the discharge of ballast would not let me out at the top of the cloud, nor the discharge of gas out of the bottom of it, though I had expended at least thirty pounds of the former in the first attempt, and not less than a thousand cubic feet of the latter, for the balloon had also become perforated with holes by the icicles that were formed where the melted snow ran on the cords at the point where they diverged from the balloon and would, by the surging and swinging motion, pierce it through.

"I experienced all this time an almost irresistible inclination to sleep, notwithstanding a nauseating feeling . . . and the terrible predicament I was placed in, until, after eating some snow and hail mixed, of which a considerable quantity had lodged on some canvas and paper lying in the bottom of the car, I felt somewhat easier in mind and body (for it is no use to say that I

cannot be agitated and alarmed), and I grasped a firm hold of the sides of the car, determined to abide the result with as much composure as the nature of the case would admit; for I felt satisfied it could not last much longer, seeing that the balloon had become very much weakened by a great loss of gas. Once I saw the earth through a chasm in the cloud, but was hurled up once more after that, when, to my great joy, I fell clear out of it, after having been belched up and swallowed down repeatedly by this huge and terrific monster of the air for a space of twenty minutes, which seemed like an age, for I thought my watch had been stopped, till a comparison of it with another afterwards proved the contrary. I landed in the midst of a pouring rain, on the farm of Mr. Goodyear, five miles from Carlisle, in a fallow field, where the dashing rain bespattered me with mud from head to foot as I stood in my car looking up at the fearful element which had just disgorged me.

"The density of this cloud did not appear alike all through it, as I could at times see the balloon very distinctly above me, also, occasionally, pieces of paper and whole newspapers, of which a considerable quantity were blown out of my car. I also noticed a violent convulsionary motion or action of the vapor of the cloud going on, and a promiscuous scattering of the hail and snow, as though it were projected from every point of the compass."

In this extraordinary and unique experience are contained matters of instruction and suggestion, some already included in our theory of local storm formation; some not yet included. For example: Are the primary phenomena of a thunder-storm confined to the cloud stratum? Are the familiar phenomena which we see at the earth surface only secondary? Was the intense cold within the cloud confined to the cloud space, the air above and below being but little disturbed from its normal condition? Is the horizontal whirl confined to the storm cloud, and is it so small as to gyrate a balloon in its axis? Is a vortex, as a whole, confined to a space limited by the cloud and so completely isolated and self-contained that, notwithstanding the escape of gas, the free use of ballast, and the skill of the aeronaut, it held the balloon in its unrelaxed grasp for eight or ten circuits, finally throwing it out by mere excess of momentum? Is that really the way in which the successive layers of hailstones are built up; and are they only released when they have such an excess of momentum, or when the energy of the vortex begins to be exhausted? And in general is the thunder-storm really a terrific localized struggle in the upper air, the intensity

of which is only indicated faintly to us at the surface? Or must we accept for this able, experienced, and apparently conscientious aeronaut a large personal equation and highly creative imagination?

There are other instructive suggestions in John Wise's book, and he is only one of a long list of men who have made observations in balloon voyages which contain meteorological suggestions. Robertson, Gay-Lussac, Barral, Bixio, Welsh, Glaisher, Flammarion, Tissandier, Fonvielle are prominent and well-known names in this long list, in which too are some American names,—Rittenhouse, Hopkins, Jeffries, Wise, Donaldson, Rotch, Hazen, and Hammon. The list is now rapidly growing, and aeronautic societies are organizing in all parts of the world.

We owe much to these numerous explorers of the air. No discount can be made from their devotion to science; and the harvest of facts which they have collected contains many grains of the highest interest and importance.

There is, however, one unfortunate feature of all these attempts to explore the upper atmosphere, an objection which does not detract in the slightest from the credit of the explorers, or the accuracy of their work, an objection that is incidental to the stage of progress that this exploration has reached, and that is, that these observations are essentially sporadic, occasional, unsystematic. Now systematic work is the method of progress in all the sciences; even in the experimental ones, where all the conditions can be created under the hand of the experimenter, the systematic prosecution of his work is the only one which permits him to reason conclusively on his results. Still more is this true of the sciences of observation, and most of all is it true where the conditions are continuously and rapidly changing as in astronomy and meteorology. The rapid advance in astronomy began with the foundation of observatories and the consequent systematic observations, and so far has this gone that even the solitary observer now has the results of observations elsewhere condensed and published for him annually in the Nautical Almanac. Modern meteorology began its advance with systematic observations, but the rapid advance of the last quarter of a century took place only by a still farther systematization, viz.: that of simultaneous observations permitting the construction of the weather map. The observations

thus far taken in the upper air are essentially sporadic and occasional. What is now needed is to make them systematically or on some well-digested plan.

There is great need of this at the present time. The harvest of conclusions to be drawn from the weather map, is now well-nigh exhausted. Further results will follow, but they will be slow, and the progress painful. The deductions from the weather map are numerous and important, and on them are founded our present rules of forecasting. That the art is not perfect, is a truth of which many of us are painfully aware. It is not too much to say that the art of weather forecasting has not advanced beyond the Ptolemaic stage, if it is not indeed yet in the early Chaldean stage Even with the elaborate and infinitely complex Ptolemaic theory of the universe the position of the heavenly bodies could be predicted for months or even years ahead with more precision, than can be reached for the storm centres for as many days. The reason of the imperfection of meteorology is the lack of exact knowledge as to what is going on in the free air. We are tied down to the bottom of our aerial ocean; and a stratum two hundred and fifty feet thick includes all we know with anything like exactness, except for the solitary station of the Eiffel Tower, which extends to one thousand feet. We can reason and draw conclusions about what occurs at greater heights; but we need direct knowledge by observation for the stratum twenty thousand feet thick. This can only be obtained by systematic aerial observations and it can be thus obtained very rapidly. We are now in the pre-Ptolemaic stage of progress in weather forecasting. It took astronomy two thousand years to pass from this stage to the Newtonian stage. If meteorology is to continue as now, it may take it as long to pass from empiricism to correct theory. If systematic exploration of the air is undertaken we may fairly hope to abridge the two thousand years to a single generation, and enormous advances can be made in a single year.

The atmosphere is one of the great laboratories of nature and the chief work in this laboratory is apparently carried on in the cloud layer from two thousand to ten thousand feet above our heads. The surface of the earth clogs these operations by friction and by highly diversified and limited local abnormalities of temperature and circulation. The smaller feat-

ures of the surface — the street, the ploughed field, the grove — cause conditions affecting our comfort far out of proportion to their meteorological importance. They play no part in the action and reaction of cyclone and anti-cyclone, and we give them a fictitious importance. It is only the larger features of topography that play an appreciable part: an ocean, the backbone of a continent, a great river basin,— these play a part in the interaction referred to, yet observations in such places give us little information of what goes on five thousand feet above. The mountain observatory gives us results of the highest interest, but still surface observations. They are different from those at the base of the mountain, but they are also different from those in the free air. The petty local conditions still play their part, and it is as unsafe to reason from mountain results to free air results, as to reason from results on the plains to those in free air. Our chief agency in investigating the upper air from stations on the surface is the clouds. Their study has led to many important conclusions, but there is an incompleteness about these conclusions due to some uncertainty as to the heights of the clouds. The lower layer, too, may obscure those beyond, and the apparent cloud motion is the result of both actual motion and that due to the extension of the conditions under which it is formed, — two very different things which observation confounds.

Meteorological theory requires an exact knowledge of what goes on in the upper air entirely free from the surface. We know the base of a cyclone. What is at the top? What the intermediate parts? What is it 500 feet from the surface? How high is it? What is the perturbation above it, if any? We have theories on these subjects deduced from surface observations, from cloud observations, and from general principles, but we lack the actual knowledge of what goes on above. We miss the keystone of our arch of theory, although the foundations are based on fact. What is true of the cyclone, is true of each other matter of meteorological theory.

How can we bridge over the gap between the pre-Ptolemaic and the Newtonian stages? How can we bridge this river instead of taking the time and labor to go around it? There is but one way known at present, and that is by ballooning, and by doing this systematically and constantly. The art already

exists ; the men with the necessary learning and skill can be found; the only need is the sinews of war — money — to provide the materials and maintain the service. How can this be obtained? It is my belief that it can, and should come only from private sources. Many of those who are entrusted with the duty of appropriating public money believe that such an object as the exploration of the upper air is not a proper one for public expense, and there are good reasons for their belief. Besides, appropriations of public money have too broad limitations, which prevent them from attaining their ideal capacity for usefulness in purely scientific pursuits. The first is the red tape, a very proper check against misappropriation, necessary because the employer is a non-person, being the entity called by some such name as the Government of the United States, or the Commonwealth of Massachusetts. The second limitation is found in the inflexibility of the machinery for supplying the personnel for such an enterprise. It is admirably adapted for expressing the will o the people, but there is not so much certainty as in private enterprise that the man and the opportunity will meet at the fitting moment. The highest economy of time, talent, and money can be best assured in private station.

I commend this subject to your special attention. I commend it to the New England Meteorological Society, who can find the precedent made by a New Englander, Doctor Jeffries, over a century ago. I commend it to the New England Society of Applied Meteorology, because improvement in the applications of meteorology would follow at once on improvements of the theory. Personally, I know of no other direction in which the expenditure of money would give such quick, important, and noteworthy results. Not only would meteorology benefit immensely as a science, but the art of forecasting would take an enormous stride, and commerce, agriculture, and all other forms of industry be correspondingly benefited. Still further, there would be important bearings on the art of war, and the art of transportation, and further yet, America would advance to her proper place in the art of aeronautics, an art which, strangely enough, she has neglected, though it is one in which, according to the world-wide estimate of her mechanical skill, she should be pre-eminent.

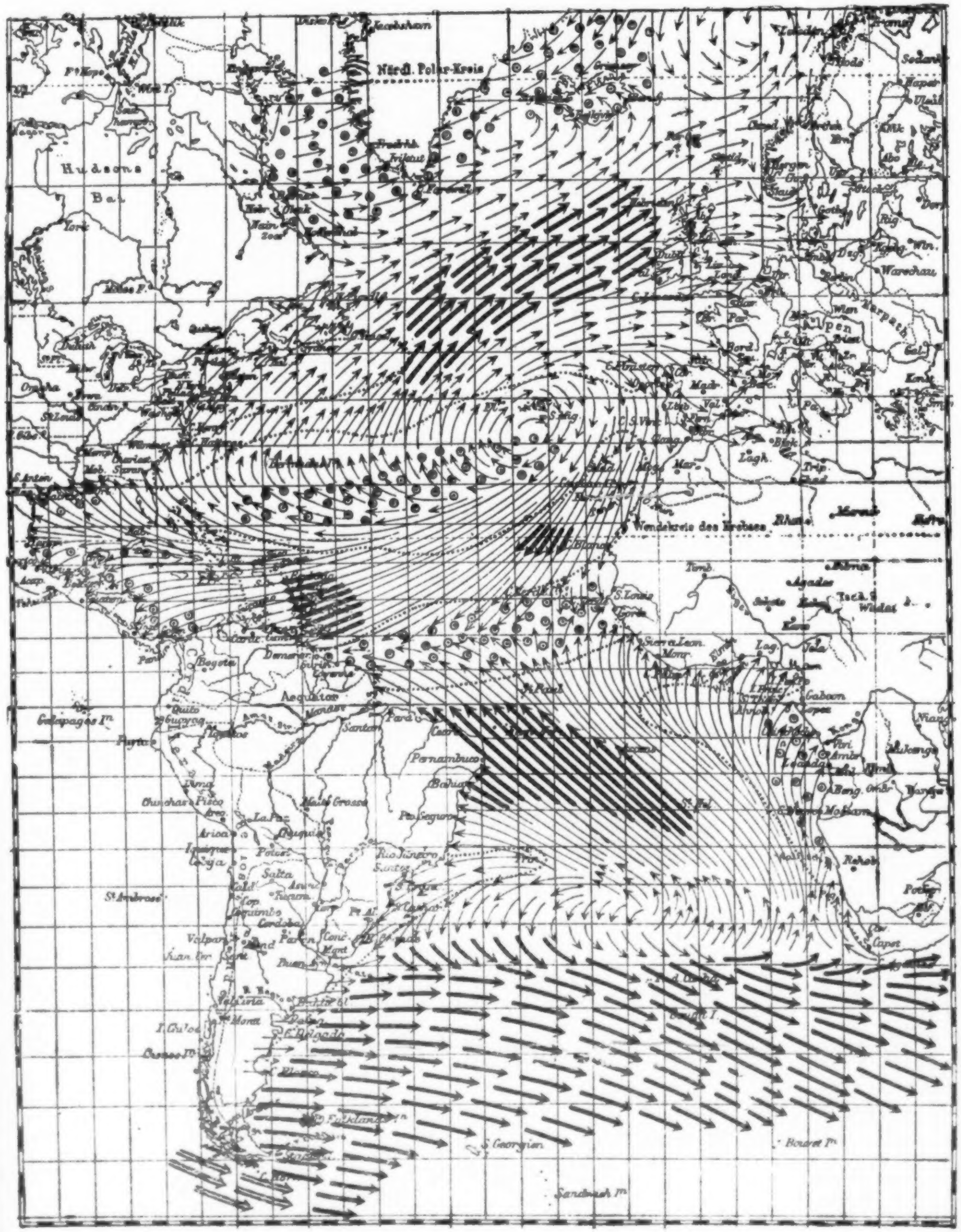
THE GENERAL WINDS OF THE ATLANTIC OCEAN.

PROF. WILLIAM MORRIS DAVIS.

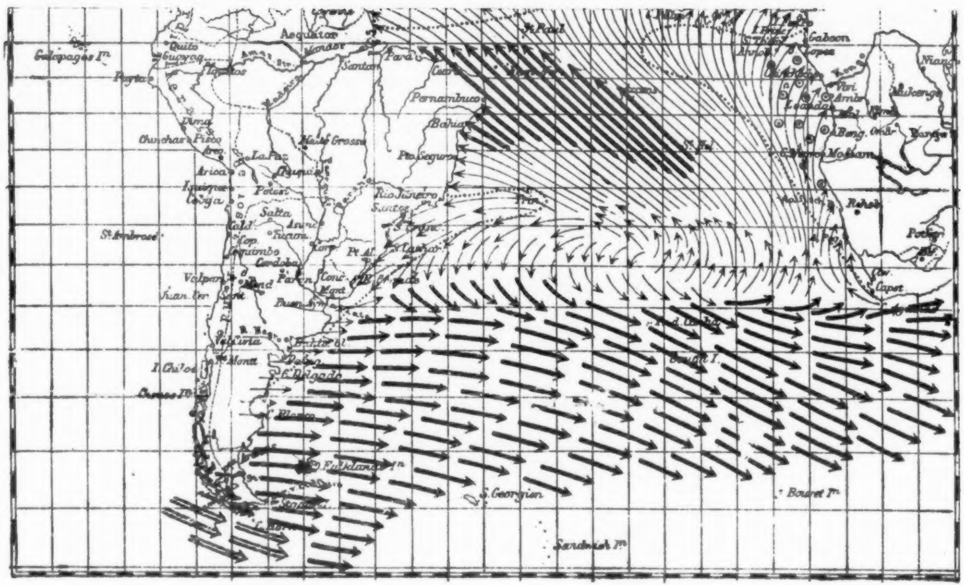
THE charts of the winds generally accessible in text books or meteorological atlases represent the general winds of the globe in a less graphic form than is obtained for the distribution of temperature by the ordinary isothermal lines, or for the distribution of pressure by the charts of isobars. Wind charts as a rule indicate the direction of the wind only at places where actual records have been obtained, and elsewhere are blank. This is as if charts of temperature and pressure were limited to the presentation of mean numerical values alone, and had no isothermal or isobaric lines. There is good reason for this difference in the difficulty of securing trustworthy records of the winds at stations numerous enough to warrant generalization; yet teachers and scholars must unite in desiring that generalization should be attempted by some competent hands from the fullest store of facts, rather than by themselves on the incomplete suggestions of the ordinary wind charts.

While the meteorological charts for the ocean, such as are issued by various government hydrographic offices,* certainly serve the purpose of the navigator best by presenting the percentage of different winds in the various five or ten degree squares of latitude and longitude, it is equally manifest that these faithful and ungeneralized charts do not serve well in elementary instruction. They are too full of detail; they do not present the leading facts with sufficient emphasis or continuity. They are well adapted to advanced instruction or investigation, but they are fatiguing to a class of beginners. The small scale charts of text books are hardly more satisfactory. It is difficult to impress a class with their meaning; the wind arrows are too scattered and independent of one another; adjacent arrows are often contradictory. Something more than the skill of a college student is wanted to discover the order that is concealed in their confusion. The most recent atlas charts — those by Hann in

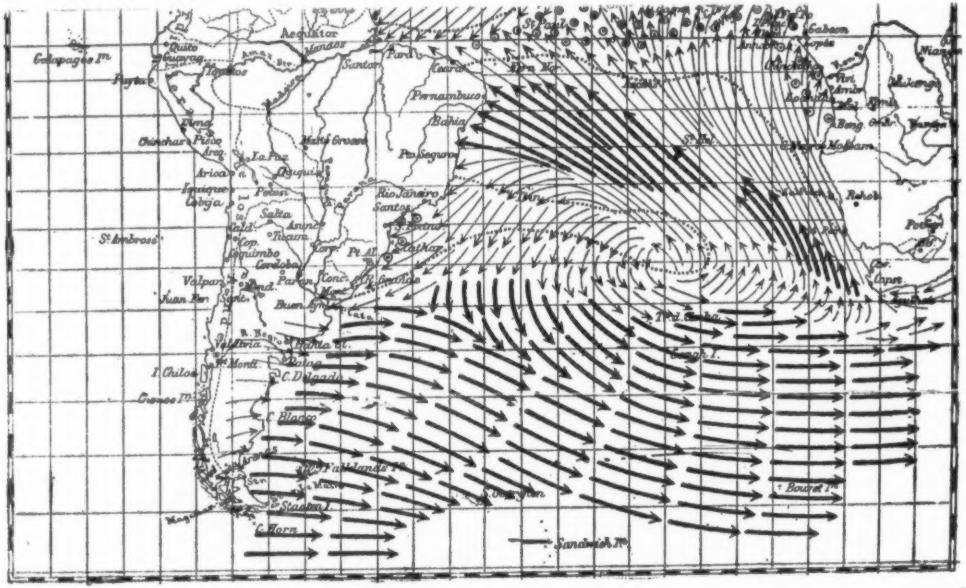
* For an account of a number of such charts, with illustrations of their method of indicating wind directions, see an article by the author in, *Science* III, 1884, 593, 654.



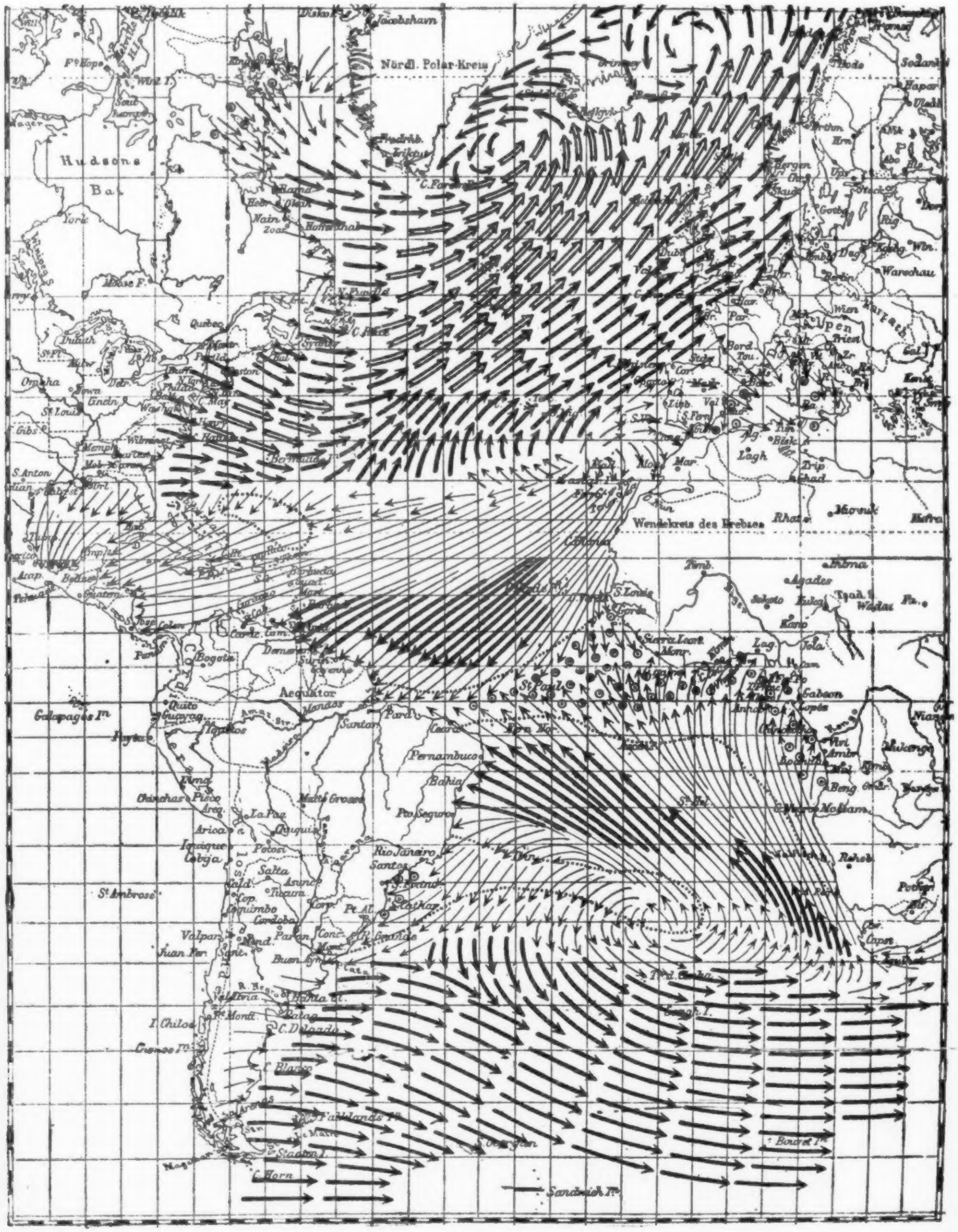
Nördl. Polar-Kreis
Hudsons Bai
Wendekreis des Krebses
Pernambuco
Bahia
Aequator
Bogota
Galapagos I.
Falkland I.
S. Georgien
Sandwich I.
Warschau
Gaboon



Galapagos Iⁿ
Quito
Payta
Lima
Pisco
Arica
Potosi
Salta
Cordoba
St. Ambrose
I. Chiloe
Santos
Para
Ceara
Pernambuco
Bahia
Rio Janeiro
St. Paul
St. Hel
Gabaon
Bouvet Iⁿ
S. Georgien
Sandwich Iⁿ



Galapagos Is.
Quito
Guayaq.
Payta
Lima
Chinchas
Pisco
Arica
Iquique
Cobija
La Paz
Potosi
Salta
Cordoba
Valpar.
Juan Fer.
St. Ambrose
I. Chiloe
Aequator
Para
Ceara
Pernambuco
Bahia
Pto. Seguro
Matto Grosso
Rio Janeiro
Santos
Buen. Ayres
R. Negro
Valdivia
C. Delgado
C. Blanco
Falklands Is.
Staaten I.
C. Horn
St. Paul
St. Hel.
Gabon
Bourel Is.
Sandwich Is.
Capst.



Hudsons Bai
Nördl. Polar-Kreis
Jacobshavn
Wendekreis des Krebses
Aequator
Bermuda I.
Galapagos In.
Bogota
Quito
Pernambuco
Bahia
Rio Janeiro
Santos
St. Ambrosé
Valpar.
Juan Fer.
I. Chiloe
C. Horn
Staaten I.
Falklands I.
Sandwich In.
Bouvet I.
Gough I.
Mattó Grosso
Arica
Iquique
Cobija
La Paz
Potosi
Salta
Pernambuco
Warschau
Wien
Alpen
Gabun
St. Paul
Sierra Leone
St. Louis

Berghaus' Physical Atlas (Perthes, Gotha) and by Buchan in his Report on the Meteorology of the "Challenger" Expedition — are great advances on any of their predecessors; yet even in these excellent maps the same difficulty of interpretation is found. They serve well the purposes of the expert who wishes to know just how far observation has gone; but they leave much to be desired for the student.

It is on this account that I wish to call the attention of readers of the JOURNAL to the accompanying copies of two generalized wind charts of the Atlantic Ocean published along with a great store of other nautical information in the "Sailing Directory of the Atlantic Ocean," by the German Naval Observatory at Hamburg * under the direction of Dr. Neumayer. These charts are here reproduced by permission of Dr. Neumayer.

Being in a foreign language, the publications of this active institution are not generally distributed in this country, although they include a variety of original essays and investigations of high value. It is precisely from such a source as this that the desired generalized wind charts should emanate. Here all materials are at hand; here well trained students are encouraged to go to the bottom of every subject that is concerned with maritime meteorology; and teachers as well as sailors have cause for congratulation that among the questions thus discussed we find the general course of the winds over the best known of the oceans, with the results strikingly illustrated in charts for winter and summer.

We may first briefly consider the facts presented in the charts; and after this examine the meaning of the facts and their relation to the general circulation of the atmosphere over the earth.

The direction of the winds is indicated by arrows. The length of the arrows indicates the relative constancy or variability of the winds. The average strength of the winds is indicated by lighter or heavier lines; the double-lined arrows meaning an average of more than six on the 12-part Beaufort scale, or over twenty miles an hour. Calms are shown by little circles. A dotted line includes the areas where the average wind is less than four on the Beaufort scale, or under about ten miles an hour.

* Deutsche Seewarte. Segelhandbuch für den Atlantischen Ozean, mit einem Atlas von 36 Karten. Hamburg, 1885.

We thus find calms and light variable winds near the equator; steady winds of light or moderate strength in the trade belts; variable winds in the horse-latitudes, with calms in those of the North Atlantic in July and August; generally strong but variable westerly winds in the temperate zones, their strength being comparatively constant through the year in the southern hemisphere, but increasing distinctly from summer to winter in the northern hemisphere; and finally two persistent cyclonic areas, one on either side of Iceland in the high northern latitudes, with light winds and calms in summer and brisk winds in winter.

The difference between the two seasonal charts is not only in the varying strength of the winds, but also in the location of the several divisions between the different members of the wind system. The doldrums, or equatorial calms, shift north and south following the sun, although at no time extending south of the equator. The belts of light and variable winds in the horse-latitudes not only migrate with the season, but undergo a certain change of arrangement, particularly in the northern hemisphere; here the separation of the trades from the prevailing westerlies is by a somewhat linear division in winter, while in summer these two members become confluent with one another, thus forming an anticyclonic whirl around the Azores as a centre.

The relation of the winds to the adjacent continents of the northern hemisphere also shows marked variations with the seasons. In summer the trade winds turn northward in the Gulf of Mexico, and flow over our Southern States; and the winds of the temperate zone at this time are from the southwest, flowing along our eastern coast. In winter the winds flow off of our coast almost at right angles, becoming northwesterly along the Atlantic coast and northeasterly in the Gulf. On the western coast of Europe there is again a contrast between oblique coastwise winds in winter, and inflowing winds in summer. When these changes are looked at in connection with those mentioned under the horse-latitudes, they will be seen to accord with one another very closely. The right-handed curvature of the summer westerlies accommodates itself to the anticyclonic whirl around the Azores, and the left-handed curvature in winter aids in the definition of the linear separation between the westerlies and the trades.

The facts thus summarized may be further considered along two very different lines of thought. One is the directly practical line, which properly seeks utility in the result of the vast amount of observation and study that has been consumed in the preparation of such charts as these. The other is the scientific, or as some would call it, the theoretical, which looks for the causes of the facts, whether of use or not, simply for the satisfaction that comes from the understanding and appreciation of natural phenomena. Although it may not be the common practice for the same mind to follow both paths, it is fortunately quite possible for it to do so. There are sometimes sailors who look beyond the use of the facts to an appreciation of the causes, as well as landsmen who enjoy the increase of utility that follows fuller knowledge ; although there are not so many of either of these classes as one might wish. Along the first of these lines, the logs preserved in our hydrographic office, the published extracts from captains' reports in the "Nautical Magazine" of London, in the "Annalen der Hydrographie" of Berlin, or in the "Annales" of Paris, might supply many narratives of interest. They would tell of the region of double-lined arrows in the winter of the North Atlantic, where vessels labor along under reefed top-sails, plunging through a dark billowy sea streaked with foam in long lines following the blustering westerly wind ; or of the latitudes where a ship falls in with the steady trades, spreading all her canvas, and rigging out extra studding sails to speed steadily on her way day and night ; or of the lazy days in the doldrums, the sea gray and glassy, heaving in long slow swells, the sails flapping from side to side while waiting for the first light breeze to carry them a stage further across the line ; but as we are here devoted to meteorology rather than to navigation, it is proper enough that our pages should be filled chiefly with a consideration of the second line of thought, namely, with the relation of the Atlantic winds to the general circulation of the atmosphere and of this to its causes.

Several years ago I presented a classification of the winds in this JOURNAL, using variety of cause as the basis of arrangement. The Atlantic winds here charted give admirable illustration of the classes termed planetary, terrestrial, and general, which may now be considered in order. In a later article, I shall give especial attention to an illustration of still another

class, the monsoons of the continental winds, taking as my text the generalized winds of the Indian Ocean from another atlas, also published by the German Marine Observatory.

Planetary winds are those possessed by any planet having an atmosphere, revolving on its axis, and warmed around the equator by a sun, but independent of other minor controls. Change of seasons and alternations of land and water surface will be considered under later headings. The essential features of planetary winds are very simple. Easterly trade winds between the tropical belts of high pressure flowing obliquely towards the low pressure equator; extra-tropical westerly winds flowing obliquely eastward around the still lower pressure of the poles; calms and light winds under the low pressures at equator and poles, and under the high pressures at the meteorological tropics, or the division between the trades and the westerlies, commonly called the horse latitudes. With exception of the facts concerning the poles, these features are well illustrated in the winds of the Atlantic.

It seems to me that the true appreciation of the trades and the westerlies can be gained only by considering them as essential features of a planetary circulation; not simply as winds occurring in certain latitudes on the earth. We are all persuaded of the universal application of the law of gravitation; of the action of radiant energy and of heat on gases; and of the deflection of movements on rotating globes; hence the propriety of studying out the features of an ideal planetary circulation in preparation for an understanding of the actual winds of our own globe.

I trust that it is unnecessary for me to remind readers of this JOURNAL of the great share that Ferrel had in giving rational explanation to this problem, and of the dominating influence of his theories in modern meteorology. I have tried to make this plain in reviews of his books; of his "Recent Advances in Meteorology" in "*Science*" for June 3, 1887, and of his "Popular Treatise on the Winds" in "*Science*" for Feb. 28, 1890; and in this JOURNAL for May, 1892, attention was called to the utter inadequacy of any theory of the general circulation of the atmosphere that omits an explanation of the prevailing low pressure around the poles, which Ferrel was first to elucidate. The mental satisfaction that comes with an understanding of this problem in meteorology is akin to that which follows a

proper grasp of the guidance of planetary revolutions by gravitation in astronomy; or of the solution of so many difficulties by the conception of the mechanical theory of heat and the undulatory theory of radiant energy in physics.

Given an atmosphere with an excess of temperature around the equator, there must be an interchanging circulation between the equator and the poles. On account of the rotation of the planet on its axis, the interchange cannot follow the meridians, but must take oblique courses; and the greater part of the atmosphere must be thrown into an immense whirl around the poles; thus reversing the polar high pressures, that would result from the low polar temperatures, to low pressures by the excessive centrifugal force of the circumpolar winds. The surface members of such a circulation must, as Ferrel showed, move in oblique courses, essentially as the actual winds move, and as they are here represented. When the rest of the world is as well charted as the Atlantic is now, we shall doubtless find a fuller representation of the planetary winds all around the earth, as is already indicated by all that is known of them from incomplete observations.

Different planets may differ in mass, and on planets of small mass gravitative motions must be slow; they may differ in period of rotation, and on those which turn slowly the obliquity of the winds will be moderate; they must differ in their supply of solar energy, and on those far away from weak suns the contrasts of polar and equatorial temperatures, on which the entire circulation depends, must be small, and the circulation must be slow. Although we have no close knowledge of the winds of other planets than the earth, yet the winds on the earth will be better appreciated if their relation to other possible wind systems is thus considered.

There is still another manner in which planets differ, and this is in the obliquity of the axis to the plane of the orbit. The special value of the obliquity in our own planet, along with the special values of the other controlling factors, gives certain definite features to our example of planetary winds; hence the special name, terrestrial winds, is given to them.

The terrestrial features of the Atlantic winds are seen in the annual migration of the wind belts north and south and in the seasonal variations of velocity and direction of the winds of either hemisphere.

The migration of the doldrums is a natural consequence of the sun's shifting north and south of the equator; the migration of the effect being much less than that of the cause, and following the migration of the cause in time and place; but it is not at first so apparent why the belt of weak winds in the horse-latitudes should migrate also. This belt is not an immediate consequence of the distribution of sunshine, but depends on the interaction of high temperatures and expansion of the air around the heat equator on the one hand, and the centrifugal force of the circumpolar whirl on the other hand. In the winter hemisphere, where the poleward temperature gradient is strong on account of the then increased temperature gradient between pole and heat equator, the winds blow faster — as is so well shown on the North Atlantic — and the centrifugal force of the circumpolar whirl is increased; at the same time, the heat equator having moved somewhat further from the pole opposes less resistance to the accumulation of air in the tropical belt of high pressure. The combined action of these two causes determines that, in the hemisphere where sunshine is weak, the winds shall go faster and the tropical belt of high pressure shall migrate towards the equator, while in the summer hemisphere, where the winds are somewhat relaxed and the heat equator approaches the pole, the tropical belt shall migrate in same direction. The whole wind system thus pulsates with the seasons. This point is generally so little understood that it is well worth attentive examination.

One of the most interesting consequences of the migration of the terrestrial wind belts is seen in the control thus exercised over the wet and dry seasons of various regions. The doldrums have rains almost daily. Where their width is greater than their migration, there will be a narrow belt always covered by their calms and diurnal convection, with rain all the year round. Where their width is less than their migration, there will be two rainy and two comparatively dry seasons, as happens on the islands in the Gulf of Guinea and on the Gaboon coast of Africa. At these truly equatorial stations the equinoxes are wet and the solstices are relatively but not absolutely dry.

The trade winds belts are relatively dry; their winds blow steadily with few stormy interruptions. On the continents they commonly produce deserts; hence the great Sahara, where

northern Africa widens out to so great an area in the trade wind latitudes. Fortunately for North America, the corresponding latitudes in the New World are occupied for the most part by the narrowest measure of our lands. As the equatorial rains move north and south, they enter regions that are for the rest of the year traversed by the drying trade winds ; here the year is divided into one wet and one dry season. The heavy rains of the Soudan when the sun is north, and of the upper Congo region when the sun is south, alternate with dry seasons for the rest of the year ; the rains in our summer on the *llanos* of Venezuela, and in the southern summer on the interior *campos* of Brazil have compensating dry seasons when the plains are arid. The belt over which the doldrums migrate may be called the subequatorial belt; and the marginal rains of this belt may be called the subequatorial rains.

The horse-latitudes are relatively dry, having a fresh, clear air with light rainfall, contrasting with the sultriness and heavy rains of the doldrums. Another contrast is found between the steady trades and the variable westerly winds, disturbed by frequent cyclonic storms from which most of the rain falls in temperate latitudes, especially in the southern hemisphere, where the temperate latitudes are so largely oceanic. The storms increase in violence in winter, and the windward westerly coasts have their greatest rainfall at this season ; but at interior continental stations, the rainfall is greater in summer, and is probably then in greater part the product of local convectional storms than of larger cyclonic storms, this being a feature characteristic of the northern hemisphere.

The belt over which dry air and light breezes of the horse-latitudes migrate possesses dry summers and wet winters ; it is known as the subtropical belt. Thus Spain, Algeria, and the Mediterranean countries in general are within or south of the horse-latitudes in summer ; they are then calm or are swept over by the parching trades, and are practically rainless. But in winter they are north of the horse-latitudes ; they are then traversed by the cyclonic storms of the westerly winds from which they receive their moderate rainfall. Hence the northern margin of the Sahara is lightly watered in winter, and the southern margin in summer ; and the intermediate belt which has no regular rains at any season is of much less breadth than is generally sup-

posed. Indeed, in certain longitudes, the northern area of winter rains almost meets the southern area of summer rains.

The subtropical belt in southern Africa is of small area; it is represented only in a district around Cape Town, because Africa extends so moderate a distance into the southern belt of westerly winds. In America, the subtropical seasonal features are much better expressed on the Pacific coast than on the Atlantic, and hence will not be considered here. Although the subject of rains is a digression from that of the winds, yet they are so closely bound together that the former should always be presented as an illustration of the latter, and not merely as a feature of the seasons to be learned as a part of statistical climatology.

The presence of continental surfaces, rising above the ocean waters, introduces the continental winds and indirectly determines many modifications in the terrestrial winds. These are naturally to be searched for chiefly in the northern hemisphere, where the continents expand to their greatest breadth. It is on account of the great seasonal variations of temperature over the broad northern lands that the westerly winds of our hemisphere show so much greater variation in velocity from winter to summer than is found in the winds of the southern hemisphere; but even in the latter there is some variation, as testified to by the appearance of double-lined arrows off Cape Horn in July and August.

If the contrast of land and water surfaces alone determined the winds, there would be inflowing winds towards the lands in summer and outflowing winds in winter; but this alternating circulation cannot be developed in its simplicity because of the greater circumpolar circulation already in existence. The greater and the lesser systems can therefore only agree to act together, one modifying the other and neither having its way alone. In this way we find explanation for the varying régime of the winds on the two sides of the North Atlantic in winter and summer. Remembering that eastern North America lies to windward and western Europe lies to leeward in the circumpolar whirl of the westerly winds, it naturally follows that the most direct summer inflow is seen in Europe, and the most direct winter outflow is from North America. The winter tendency to outflow from Europe produces only coastwise south-

west winds; the summer tendency to inflow towards North America likewise produces only southwest winds along the coast, except in the Gulf where they flow more directly towards the land.

The contrast between the strong annual range of temperature in our eastern States and the small range in western Europe follows as a corollary from the preceding statement. As we lie to leeward of a continental interior, we have cold winds in winter and warm winds in summer; but as our winter winds are from the northwest, they are extremely cold, and as our summer winds are from the southwest, they are unduly warm. As western Europe lies to leeward of an ocean, it has mild winds in winter and temperate winds in summer; but as its winter winds come from the southwest, they are particularly mild, and as its summer winds come from the northwest, they are relatively cool.

A further corollary is seen in the production of the anticyclonic whirl in the summer winds of the North Atlantic. The summer heat expands the air over the continents, and shoulders it off towards the cooler oceans, thus interrupting the continuous tropical belt of high pressure characteristic of the terrestrial winds, and breaking it up into local high pressure areas over each ocean. Outward from the centre of these areas, the wind gently flows away with a deflection proper to its hemisphere. The North Atlantic gives the best illustration of this effect in the world, because it is so well enclosed by continents.

An indirect consequence of the presence of continents — that is, a consequence not directly dependent on changes of temperature over the lands — is seen in the persistent cyclonic whirls between Norway and Greenland. These are produced by the abnormally high temperature of this part of the ocean, where so much warm water is concentrated in high latitudes, and this concentration in turn depends on the continental boundary of the ocean. The whole configuration of the Atlantic seems to conspire towards the high development of this peculiar result. The South Atlantic does not receive so much cold Antarctic water as flows into the South Pacific, because Africa does not extend so far south as South America; hence the Atlantic equatorial current in the Gulf of Guinea is much warmer than the Pacific current off Peru. The overlapping outlines of

Africa and South America determine that a large part of the warm equatorial current of the Atlantic shall be turned off from the southern hemisphere into the northern hemisphere. The great volume of warm water that thus passes along the coast of Guiana is still further warmed in its delayed passage either side of the islands of the West Indies. The Gulf Stream, to which popular attention is so largely given, makes only part of this current, and if the name is limited as it should be to the water that issues through the Strait of Florida, the current should be called the North Atlantic drift after passing Cape Hatteras, or a latitude a little farther north. Discussion over names is, however, relatively unimportant; the fact is fortunately plain enough. The great body of warm water flowing poleward on the western side of the North Atlantic eddy subdivides as it turns eastward about latitude 40° N., one branch flowing south opposite Spain and Africa to complete the normal circuit of the eddy; the other branch turning northeast along western Europe and entering the narrowing ocean of the polar regions; the only pronounced and persistent example of such a current in the world. On either side of Iceland the sea water is abnormally warm. In this region are found the most abnormally high atmospheric temperatures of the world; this is especially the case in winter, when the sun has gone south and left the control of atmospheric temperature chiefly to the land and water. Here the barometric pressures are abnormally low in consequence of the abnormally high temperatures, and around the low pressure areas as thus determined, the winds circulate in cyclonic whirls of much distinctness. The area of the whirls should not, however, be measured from the accompanying chart, which is on the convenient but distorting Mercator projection; it should be reduced to its proper dimensions by study on a globe, where it will be seen to have relatively small proportions when compared to the great circuits of the terrestrial winds.

The eddies of the ocean currents are repeated in an imperfect way by eddies in the winds around the several ocean basins. This is in part the consequence of thermal and baric conditions already described; but it is also in part determined by the mechanical obstruction of a free atmospheric circulation by mountain ranges and plateaus, in much the same fashion as that in which the continents dominate the eddies of the oceans.

The continental winds thus far considered would exist if the lands were low plains. The presence of mountain ranges and plateaus, even if they do not rise nearly to the upper limits of the atmosphere, still exerts a considerable influence on the lower winds; and when this influence conspires with others that lead to the same end, the development of an eddy-like circulation of the winds around the ocean basins becomes very distinct. As already stated, this is very clearly the case around the North Atlantic in July and August, and less clearly around the South Atlantic in January and February.

One of the most interesting consequences of the continental interruptions of the terrestrial winds — interesting especially in its theoretical bearings — is the imperfect development of the north polar low pressures, in comparison with the extremely low pressures that so persistently characterize the south polar area. The low Antarctic pressures have already been referred to the strong centrifugal force of the great Antarctic whirl, so perfectly developed in the unbroken sweep of the southern ocean.* The Arctic circumpolar whirl is so seriously interfered with by the continents that, in spite of its activity over the oceanic areas, it hardly suffices to produce lower pressure at the pole than for the average of the surrounding high latitudes; and it is distinctly insufficient to produce a lower pressure than that of persistent cyclonic areas of either the North Atlantic or the North Pacific in winter. If the continental mountain ranges were half as high again as they now are, we might find the Arctic regions a centre of distinctly higher pressure than that of latitude 60 degrees N.

* It should be mentioned that Buchan still holds to the view that the low Antarctic pressures are the consequence of the abundance of water vapor in those latitudes, and takes no account of their dynamic cause. It is difficult to understand this conservatism, when the presence of the circumpolar Antarctic winds is so well shown on his excellent wind charts; when the efficacy of a circumpolar whirl to produce low pressures was so completely proved for the earth as a whole by Newton and for the atmosphere by Ferrel; and when the inefficacy of water vapor to produce such an effect is so manifest. The presence of water vapor may directly aid in the production of equatorial low pressures, where high temperatures allow the existence of water vapor in a relatively large amount; and by thus strengthening the terrestrial circulation the excess of water vapor around the equator indirectly aids in determining the polar low pressures; but as the absolute humidity of the cold Antarctic atmosphere is less than that of the warm equatorial atmosphere, water vapor should not be regarded as direct aid in the production of low pressures in those regions where it is deficient.

The value of the wind charts here considered will not be fully appreciated unless it is remembered that they are not to be regarded at all as diagrams prepared deductively to illustrate Ferrel's convectional theory of winds, but that they are careful generalizations on facts of observation. They therefore constitute a very perfect inductive confirmation of the consequences following from the convectional theory of the general circulation of the atmosphere; and when thus used, they are excellent means of strengthening the mental discipline that is to be found in the serious study of meteorology.

HARVARD COLLEGE, September, 1892.

THE COLORS OF CLOUDY CONDENSATION.*

PROF. CARL BARUS.

1. *Introductory.* — In defining my duties as a member of the Weather Bureau, Prof. M. W. Harrington indicated that problems connected with the condensation of water from moist air were first to be considered. I was left at liberty to look at these phenomena from a general point of view, and accordingly I have commenced the study of condensation either from fusion or from vaporization, as well as from solution (liquid or gaseous). The recent theories of solution originating in the epoch making researches of van't Hoff adduce many reasons for grouping all these phenomena under a common head.†

Questions involving the solidification of a liquid at a given temperature by pressure, I have already considered at length elsewhere.‡ As the apparatus § previously used was still available, I commenced my new work by investigating the conditions un-

* Communicated by permission of the Chief of the Weather Bureau.
The cuts are taken from a forthcoming bulletin on the condensation problem and were kindly loaned by the Bureau.

† Cf. Blümcke: Zeits f. phys. chem. vi., p. 153, 1890; viii., p. 554, 1891; ix., p. 722, 1892.

‡ Barus: Am. Journal Sci., xlii., p. 125, 1891. Cf. "The volume thermodynamics of liquids," Bull. U. S., Geolog. Survey, No. 96, 1892.

§ It gives me pleasure to take the present opportunity of thanking Major J. W. Powell and Mr. Clarence King, for leaving the physical laboratory of the Geological Survey in my charge, and also Profs. S. P. Langley and G. Brown Goode, for the further use of my old quarters in the National Museum.

der which a solid is precipitated from solution. The results will be shown at some other time, their chief feature being the occurrence of a pronounced volume lag in virtue of which solution at a given temperature takes place at a lower pressure than the corresponding precipitation. §19. When precipitation eventually does occur it is partial, and in this respect the present behavior differs from the corresponding case of freezing from fusion. In other words the solution phenomena contains a clear cut example of chemical equilibrium varying with temperature and with pressure, the meteorological bearing of which will already be manifest in the course of the following paragraphs.

Turning now to the subject proper of the present paper — the passage from vapor to liquid — it is clear that as we cannot work with free air, the method applicable to the condensation problem must be so chosen as to trace the progress of the experiment at each moment throughout the course of the change of physical state. Only by producing instantaneous condensation can the conditions of a free atmosphere be simulated experimentally, and a phenomenon, independent of the surface forces at the walls of the vessel, be observed. Above all things is it therefore necessary to obtain some means of discriminating between vapor and a collection of indefinitely small water globules. Thus, for instance, the difference of specific inductive capacity for the two cases might furnish an available criterion ; but this method would present great experimental difficulties. Nor is it clear whether any inkling could thus be obtained relative to the size of the water globules. Both these desiderata are supplied within certain limits by the colors of cloudy condensation ; and it is to the description of a method of producing them at will, of interpreting color in terms of size of particle, and of making the phenomenon tributary to the problem in question, that the present paper will be devoted. If these elusive color effects can be brought thoroughly under control, then experimental method of much greater scope than the earlier observers have appreciated must necessarily result.

Incidentally I will add other allied experiments, such, for instance, as relate to electrification, etc.

2. *Literature.* — For information on the present status of the subject, I am here as elsewhere greatly indebted to Prof.

Cleveland Abbe, who, with his usual courtesy, placed his detailed knowledge wholly at my disposal.

The fundamental researches of Coulier* and Mascart,† and of Aitken,‡ though closely allied with the present work, refer chiefly to the function of dust. They are, moreover, too well known to need comment here.

With regard to the color phenomena, the first definite observations were made by Forbes§, who worked with immense high pressure steam jets. For this reason, probably, he saw only the yellow-browns of the first order.

The systematic research of the subject followed at a much later date, and is due to Kiessling.‖ His treatment is exceptionally thorough, and virtually contains all that is known of the color phenomena by exhaustion. I shall have occasion to refer to these experiments again, and here merely state that neither the original tubular form nor the later spherical form of Kiessling's apparatus is suitable for my purposes. The condensations produced by suddenly expanding dusty air (partial exhaustion) and the colors thus evoked, however startling, are necessarily fleeting and cannot be brought under control.

The subject is next taken up by R. v. Helmholtz,** chiefly with the object of developing a new method for measuring the depression of vapor tension produced by solution. The function of dust is considered incidentally at some length, and a size is assigned to the cloudy particles. Color phenomena are not specially observed until in a second paper, R. v. Helmholtz†† corroborates Kiessling's researches by the aid of steam jets, and particularly with reference to the condensation induced by electric discharges. He analyzes the opaque steam jet into zones of color, obtaining a serviceable scale. After a series of

* Coulier: Jour. de pharm. et de chim. (4), xxii., p. 165, 1875; Cf. Fortschritte der Physik, xxxi., pp. 471 and 870.

† Mascart (see Coulier) verified Coulier's work.

‡ Aitken: Nature, xxiii., pp. 195, 384, 1880; Trans. Roy. Sec. Edinb., xxx., p. 337, 1881.

§ Forbes: Phil. Mag. (3), xiv., pp. 121, 419, 1839; ibid. (3), xvi., p. 102, 1840.

‖ Kiessling: Naturw. Verein, Hamburg-Altona, viii. (1), 1884; Gœttinger Nachr., v., p. 122, 1884; "Dæmmerungserscheinungen, Chapter V., pp. 133-145, Hamburg, L. Voss, 1888.

** R. v. Helmholtz: Wied. Ann., xxvii., p. 508, 1886.

†† R. v. Helmholtz: Wied. Ann., xxxii., p. 1, 1887.

striking experiments, v. Helmholtz concludes that a disturbance of the chemical equilibrium of the surrounding medium produces a condensation effect on the jet similar to the dust effect. Omitting further reference to this important research for the present, I shall proceed with the corroborative paper of Bidwell's.* He suggests that the occurrence of an opaque jet is due to electrical influences similar to those by which Lord Rayleigh† produced a coalescence of scattering water jets. Hence Bidwell believes the particles of an opaque steam jet to be larger than the corresponding particles in ordinary jets.

In the recent paper on the subject, Mr. J. Aitken ‡ argues soundly enough that Bidwell's view cannot be correct, without, however, giving due credit to R. v. Helmholtz,§ who had proved the occurrence of smaller particles in the opaque jet as compared with the ordinary jet some five years before. After classifying the various methods|| by which the appearance of a steam jet may be made to pass from the ordinary to the abnormal aspect, Mr. Aitken discusses the causes underlying each experiment in detail. An important novelty, namely, the effect of the temperature of the medium surrounding the jet, is here introduced. To explain all the phenomena, a repulsive force acting between the particles below a certain temperature is postulated in addition to the repulsive action of the electrically charged particles. In the second part of his paper Mr. Aitken takes up the colors of

* Bidwell: Electrician, xxiv., p. 146, 1889; Phil. Mag. (5), xxix., p. 158, 1890.

† Rayleigh: Proc. Roy. Sec. Lond., xxviii., p. 406, 1879. According to G. Wiedemann (Beiblatter, xiv., p. 291, 1890, and Wiedemann's Electricitat, i., p. 27, these phenomena were observed by Father Gordon of Erfurt, about 140 years ago. They were frequently repeated by Fuchs in 1856 and studied by Beetz in 1871.

‡ Aitken: Proc. Roy. Soc. li., March, 1892, p. 408

§ R v. Helmholtz: Wied Ann., xxxii., page 2, 1887. After referring to Kiessling's results, v. Helmholtz remarks: "Da letztere (Diffractionsfarben) von der Græsse und Regelmæssigkeit der Nebeltrœpfchen abhængen, so liegt der Schluss nahe, dass durch die electrischen Kræfte die Condensation . . . beschleunigt wird. Die Art, wie sich die Farben mit der Elektrisirung der Spitze ændern, scheint dies zu bestætigen. Ist næmlich die Menge der ausstrœmenden Elektricitæt sehr gross, so wird der Strahl blæulich bis tief Azurblau, wie der Himmel. Diese Færbung deutet auf sehr zahlreiche, sehr kleine, und sehr regelmæssige Nebeltrœpfchen hin. Lasst der Strom der Elektricitæt allmælich nach, so wird dass Blau immer weisslicher, was auf das Hinzutreten græsserer Tropfen deutet," etc.

|| These are: (1) Electrification of jet, (2) An increase in the number of dust nuclei, (3) Cold or low temperature of the air, (4) High pressure of steam, (5) Obstructions and rough nozzles. Aitken, l. c., p. 409, *et seq*.

cloudy condensation and gains an advantage by passing the steam jet through tubes illuminated by transmitted light. Unfortunately he does not perfect this arrangement, but proceeds to produce these phenomena by the exhaustion method, virtually repeating Kiessling's work, who is not even mentioned. This procedure is somewhat contrary to scientific usage, and throws a slur over Mr. Aitken's fascinating paper.

This hasty survey will suffice for the present, as more specific information is to be brought forward in the course of the following paragraphs. I have omitted references to atmospheric phenomena, such, for instance, as are discussed by Kiessling (l. c.) and Abbe,* because they do not fall within the limits of the present paper.

3. *Disgregation.* — Incidentally I will have to refer to the scattering of particles from the service of a very hot metal, as well as to the analogous phenomenon produced by an impingeing beam of ultraviolet light by electrification, etc. The work which has been done on these subjects is, however, very extensive, so that only a mere mention † of prominent papers can be made here.

APPARATUS.

4. *The Jet.* — To answer the requirements of §1, the endeavor must be made to construct an apparatus by which any of the colors of cloudy condensation may be produced at pleasure, and be maintained at the greatest brilliancy possible for any length of time. In the color estimations, furthermore, it is desirable, if not necessary, to have a given normal or fiducial case of condensation constantly at hand, for comparison with the results obtained in any variation of experiment. §9. Only in the case of vapors issuing from a jet are the preliminary

* Abbe: Am. Meteorological Journal, v., p. 529, 1889.

† The reader is referred to Nahrwold: Wied. Ann., xxxi., p. 467, 1887; xxxv., p. 107, 1888. Berliner: Wied. Ann., xxxiii., p. 289, 1888. Wiedemann and Ebert. Wied. Ann., xxxiii., p. 241, 1888. Elster and Geitel: Wied. Ann., xix., p. 588, 1883; xxvi., p. 1, 1885; xxxi., p. 109, 1887. Guthrie: Phil. Mag. (4), xlvi., p. 257, 1873. Koch: Wied. Ann., xxxiii., p. 454, 1888. Kayser: Wied. Ann., xxxiv., p. 760, 1888. Hertz: Wied. Ann., xxxi., p. 983, 1887. Stoletow: C. R., cvi., p. 1149. 1888. Hallwachs: Wied. Ann., xxxiii., p. 301, 1888. Lenard and Wolf: Wied. Ann., xxxvi., p. 443, 1889. Righi: Mem. Acad., Bologna (4), ix., p. 369, 1888. Crookes: Nature, xliv., pp. 212, 455, 1891. Prof. A. W. Wright, of Yale College, investigated this subject extensively at a much earlier date.

conditions fulfilled; for here the particles are supplied as fast as they are removed out of the field of view; and they may be removed before they change in size appreciably. §15. The jet, moreover, may either be fed with pure steam, or with compressed air supersaturated with aqueous vapor. §17.

When steam is used alone, it must be available in large quantity, at a pressure of about one atmosphere or more. This pressure may then be reduced by an ordinary steam cock. To dry the steam, it is conveniently passed into a cylindrical box, *A*, Fig. 1, through the vertical pipe, *a*, surmounted by the cock specified. It is conveyed to the jet by a lateral pipe, *b*. The water which collects is discharged from time to time through *c*, at the bottom of the box, and a pipe, *d*, communicates with an open mercury manometer for the measurement of pressure. It is often necessary to have two afflux pipes *b*, one of which is provided with a suitable stopcock. §9. *A* may be made of stout tin-plate, thoroughly soldered.

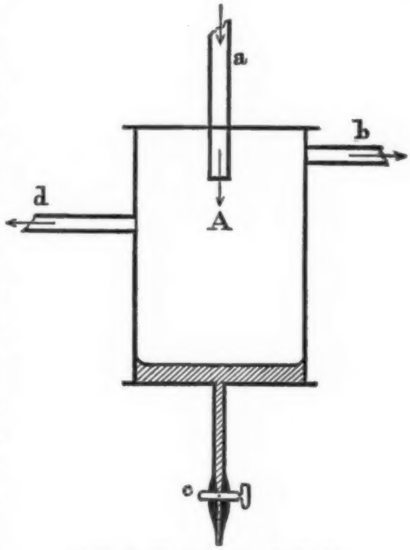


Fig. 1.—Steam Box. Scale ¼.

A jet of almost any kind, with a smooth hole about 1 mm. in diameter, is satisfactory. It may be conveniently shaped from lead pipe. When the variations of the color phenomena are to be quantitatively studied, it is advisable to make the jet as shown in Fig. 2. Here *ab* is a brass tube about 10 cm. long and 1 cm. in diameter, the end *b* of which is threaded both on the outside and inside, so that a small thin walled nipple, *c*, closed at the outer end, may be inserted. Into the top of *c*, a hole, *d*, is smoothly drilled. The outer thread at *b* is useful in screwing the jet to the color tube (§§ 5, 7), into the centre of which *c*

projects. The open end, *a*, of the jet is to be connected with the tube *b* of the steam box, Fig. 1, by a short piece of stout rubber tubing.

If this jet is so made, the diameter of the hole *d* is accurately known, and hence the quantity of steam discharged into the color tube can be computed by Napier's or Grashof's formulæ for any pressure in *A* (Fig. 1). Furthermore the amount of adiabatic cooling of the steam issuing at *d* is also easily calculated.

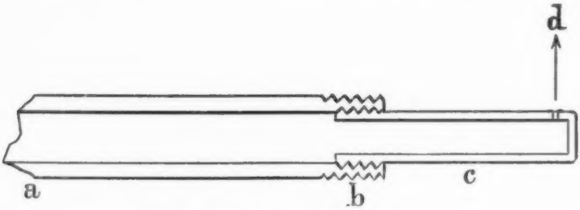


Fig. 2.—Steam Jet. Full size.

Both of these quantities are of importance when the color phenomena are to be systematically studied.

Finally, by varying the number of holes in *d*, an increased intensity of some of the very faint colors, or a peculiar persistence of others, is often obtained. §17.

Physically, the jet is virtually a region at which steam passes suddenly from a high temperature to a relatively low temperature. A mass of steam is thus continually maintained in a definitely supersaturated condition. This state of things is accentuated by the cold air with which the jet comes in contact.

6. *The color tube. First Form.*— In making the observations, the chief *desideratum* is a closed receptacle with a clear field of view from which all extraneous light is, so far as possible, exclude If a tube closed with glass plates is used, these at once cloud over when exposed to steam, and the tube is then worthless for observation, except for the loudest of the colors. To look directly into steam is equally unsatisfactory, besides being a severe tax on the eyes. One way of overcoming the difficulty is to heat the plates with a Bunsen burner above 100° C. If the glass is poorly annealed, a smooth piece of wire gauze may be stretched tightly over it to obviate breakage. All this is inconvenient. A special steam jacket at the plates is inefficient,

while tubes open at one end would not meet the requirements of the quantitative work. Hence I proceeded as follows: —

The color tube is shown in its various forms in Figs. 3, 4, 5, 6. In all these cases the tube proper *C*, *A*, *A*, *B*, is the same; but it is placed in different positions to obtain different

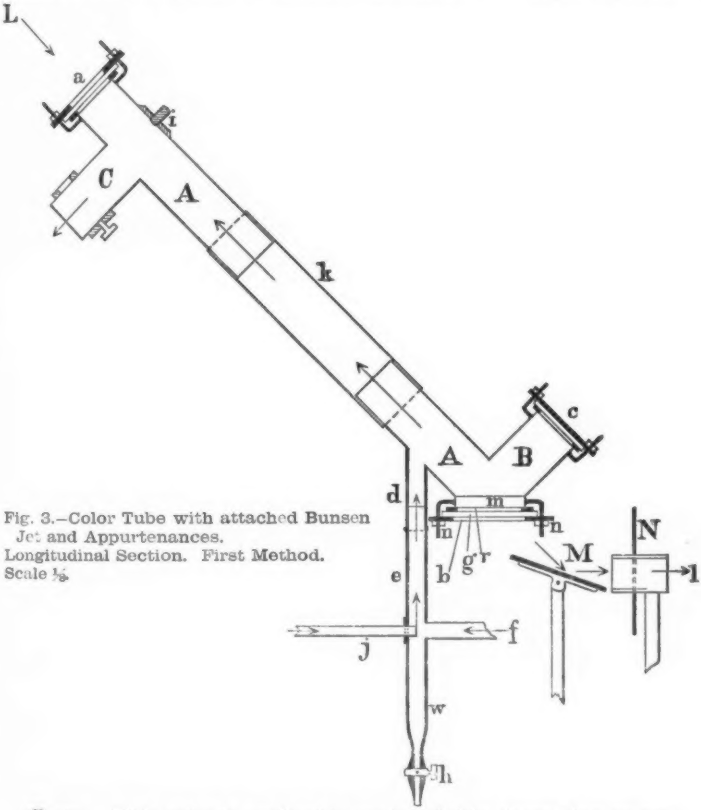


Fig. 3.–Color Tube with attached Bunsen Jet and Appurtenances. Longitudinal Section. First Method. Scale ⅛.

effects. This tube is preferably made of thin sheet zinc or copper to prevent rusting, and the part *A A*, is about 60 cm. long, and 5–7 cm. in diameter. A lateral branch *B* of the same diameter and about 10 cm. long enters the main tube at right angles, at the lower end (Fig. 3), and the common bottom *m*, of the two tubes is closed by a plate of clear glass *g*, held in place by the brass ring *b*, and the nuts *n*. This joint is made quite

tight by a gasket of rubber *r*, forced against the common rim of the tubes by the nuts just mentioned.

In the same way the top *a* of *A*, *A*, and the outer end *c* of *B* may be closed, as the figure clearly shows, the efflux pipe *C* does not need an attachment.

The mixture of steam and air (obtained in a way presently to be shown) enters near the bottom of *A A*, through another lateral tube *d*, whose axis is in the plane *A B* and nearly vertical when the plate *g* is horizontal. The vapor, after passing through *A A*, is discharged by a large tube *C* near the top.

In the position of Fig. 3, sky light, *L*, enters the tube at *a*, and after passing through the steam and the window *g* is conveniently observed by aid of a little mirror *M* and a screen *N*, with a circular hole in front of *M*. This device shuts off all light except the rays *l* from *a*. *B* is closed by an opaque plate. When the steam passes through *A A*, the window *a* soon clouds over; but this is no great disadvantage, for *a* is a mere source of diffuse white light. The window *g*, however, must be quite transparent, otherwise the colors of the steam could not be seen. To secure this end I poured a little water into the trough *m* at the bottom of the tube. A beautifully clear field is thus obtained, the observer looking as it were through a plane parallel plate of water. Additional water which collects here from the steam is discharged through *d*.

The feeder in this case resembles a Bunsen burner supplied with steam instead of gas. The jet proper (shown at *j*) has the form given in Fig. 2, and is screwed into the brass tube *e*, the top of which fits snugly into *d* of the color tube. Any water which collects here falls into the prolongation *w* and may be discharged through the stopcock *h*. When the jet is in action, air enters through lateral tubulures *f*, only one of which is shown in the figure.

This apparatus is frequently convenient because the jet *j* can be so easily supplied with pure air, or with air impregnated with dust or vapor in any manner. It shows a fair succession of blues and (under special treatment) intense yellows and orange-browns of the first order. The field becomes quite opaque with the merest trace of dust, etc. But, as a rule, the colors are not brilliant; nor can the purples and blues of the second order be seen with certainty. I was for a long time at a loss to ascertain

the cause of this, and after spending much time on useless modifications, I finally found that the amount of air which the jet *j* can drag through the tube *E*, 2 cm. in diameter, is usually insufficient to make the colors glow. Finally, for a high pressure influx, the water in *m* is apt to become turbulent.

7. *The color tube. Second form.* — Hence I made a radical modification by discarding the old form of jet, and inverting the whole arrangement in the manner shown in figure 4. Steam now enters the tube *A A* directly by the jet *j* screwed into it near the bottom. Air is supplied through *C*, which is of the same diameter as *AA*. Moreover, *B* is now the efflux, the plate having been removed. Sky light, *L*, transmitted through the tube by means of the adjustable mirror *M* and the window *a*, is observed through the inclined plate *g*. Here a difficulty presents itself inasmuch as *g* at once clouds over; but I eventually overcame this by moistening the inside of *g* with a solution of caustic potash applied through *B* with a sponge probang. In this way a clear field is again obtained for some time at least, after which the moistening must be renewed. The inclined position of *g* makes the whole window easily accessible, and a special hole closed by a cork may be cut in the elbow for inserting the probang when *B* is otherwise engaged. The tube *d* is also closed with a cork.

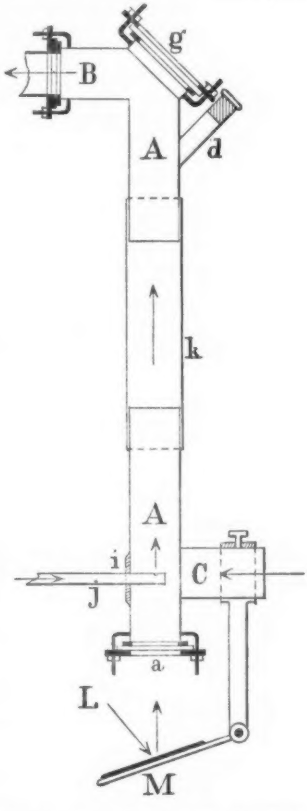


Fig. 4.—Color Tube with attached Jet. Longitudinal Section. Second Method. Scale 1/8.

The observer faces the plate *g* symmetrically, and looks down into the color tube *AA*, while a screen *S* cuts off all extraneous light.

With this apparatus a brilliant field of color is obtained so that minute differences may be detected, and I thus investigated the scale given below, § 10, between the intense oranges of the first order and the final greens of the second order, after which the colors are too faint for recognition. In the latter case, since low pressure steam must be employed, an advantage is secured by drilling three or five holes in the jet *j*.

Fig. 4 shows that the tubes *AA* slide into a snugly fitting envelope *k*. The system may, therefore, be shortened for observing very intense colors, or lengthened for faint colors. § 17. Either end of *AA* may also be rotated, which in many experiments is necessary. § 15.

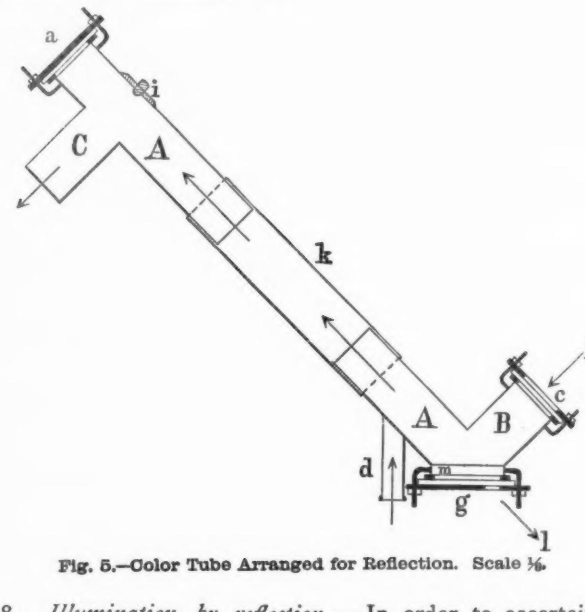


Fig. 5.—Color Tube Arranged for Reflection. Scale ⅙.

8. *Illumination by reflection.* — In order to ascertain the source of color in these experiments it is desirable to test the cloudy condensation by reflected light as well as by transmitted light. This may be done by placing the color tube as shown in

figure 5, where the steam from the Bunsen jet enters at *d* and passes out at *C*, as in figure 3. The plate *a* is now made opaque, and the plate *c* clear; while the plate *g* is replaced by a mirror, reflecting upward, and which has a little of the amalgam scraped away from the lower surface. Hence an observer, regarding the tube from below, may look in the direction of the axis *AA* upward. The mirror *M* and screen *N* of figure 3 may also be used. Figure 5 shows that sky light, *L*, now enters through *c* and is reflected by *g* parallel to the axis of the tube, a condition which may be tested by temporarily opening *a*. If the inside of the tube is painted dark, no light can reach the eye except in so far as it is reflected (*l*) from the cloudy particles introduced into *AA* through *d*.

A plate of water *m* above *g* is necessary here as in figure 3 to obviate clouding.

A similar result can be secured by introducing a little mirror on a stem through *C* in figure 4 and shutting off the light at the top of the tube. An observer looking into *C* may then view the color effects along the axis of *AA*.

9. *Differential apparatus.* — Color tests are not as a rule satisfactory for quantitative work, unless some ducial field of color is always at hand for comparison. For this reason the apparatus (figure 4) was further improved as shown in figure 6, in front view. The parts are lettered as above. Figure 6, therefore, consists of two color tubes identical in every respect and admitting of an independent use of both eyes. Sky light is intro-

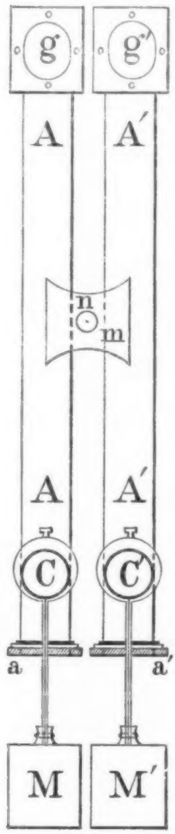


Fig. 6.—Differential Color Tubes. Front view. Scale ⅛.

duced by the mirrors $M\ M'$. The observer looks down into both windows g, g' at once. The two tubes are connected rigidly by the double bridge m, and a horizontal rod, or stout tube n, joining the bridges is grasped by an ordinary clamp for fixing the system in place.

If the jets are identical, and communicate through identical lengths of tube with similar tudes b (figure 1) of the steam box A, the colors seen by both eyes must necessarily be identical. This state of affairs may be tested once for all. Supposing therefore that the right tube $A'\ A'$ is left without interference, the other tube, $A\ A$, may be manipulated in various ways. Thus one may change the temperature, or the pressure, or the composition of the influx of air or other material at C; or the number and size of the holes in the jet may be varied, or the steam variously impregnated with other gases before it escapes at the jet, etc.

In all such cases the tubes $A\ A$ and $A'\ A'$ are connected *in multiple arc*, as it were. But if the jet be removed from $A'\ A'$ and the discharge from $A\ A$, after passing through a suitable length of wide tube, be eventually passed through $A'\ A'$ without further interference, then these two tubes are connected *in series*. This adjustment will I believe, lead to important applications of the present apparatus.

Reviewing the experience of this section it appears that the adjustment of air and steam which secures the clearest field in a color tube must be made as nicely as the adjustment of air and gas in a Bunsen burner. Otherwise the colors are either dull or faint. Doubtless the efflux may at times be stimulated to advantage, by an auxiliary ventilating tube containing its own jet, without interfering with the color tube itself. §§15,17. In using the differential apparatus it is convenient to prolong one of the air tubes, C, above the head but within easy reach of the observer.

METHODS AND RESULTS.

10. *The succession of colors.* — The first question to be disposed of is the succession of colors. Here I am able to proceed with considerable certainty, seeing that there is a rich harvest of experimental material available. I will arrange these colors in a table, stating briefly how they were obtained and

placing the colors where I think they belong. I will also add Quincke's * revision of the colors of Newton's rings, when seen normally by transmitted white light, as quoted by Kohlrausch†, for comparison.

Kiessling ‡ was the first to call attention to the striking similarity at the order of colors at the centre of his field, to certain Newtonian interferences ("gewisse Newton'sche Interferenzfarben"). After him Aitken § points out the same fact more at length, but again without reference. The remarks of these gentlemen, however, are cursory and vague; thus Aitken believes the colors to be interferences by reflection of the second and third order, whereas my results show interferences by transmission of the first and second order, terminating at the third.

* Quincke: Pogg. Ann., cxxix., p. 180, 1866.

† Kohlrausch: Leitfaden, 5 Ed., p. 340.

‡ Kiessling: "Dæmmerungserscheinungen," p. 140, 1884.

§ Aitken: l. c., p. 433: "As no white follows the first blue it seems probable that the first spectrum . . . is not observed; . . . ". But this is the property of the first order of reflected interferences only, as stated in the text.

TABLE SHOWING THE SUCCESSION OF COLORS.

(Data of the last column for air, white light, and normal incidence.)

By Exhaustion.			Experiments with Jets.				Colors of Newton's Interferences.	Thickness of plate of air in millimetres.
Kiessling. 1884.		Aitken. 1892.	Helmholtz. 1887.	Aitken. 1892.	Barus. 1892.	Present Method.		
					White.	§ 6, Fig. 3 with sulphur. § 17, moist gas jet.	White.	.000,020
		Pale yellow.		Yellow.	Yellow.		Yellow white.	.000,048
							Brown white.	.000,079
	(Second, third and fourth series.)				Orange.	§ 9, efflux violent.	Yellow brown.	.000,109
		Slightly reddish.		Brown.	Brown.		Brown.	.000,117
					Opaque.	§ 9, efflux noisy.	Red.	.000,129
(First series.)	Pale purple.			Opaque.	Opaque.		Carmine.	.000,138
					Opaque.		Dark-red brown.	.000,137
Pale lilac. Pale blue violet.					Dark violet.	§ 9, efflux intense.	Dark violet.	.000,140
					Indigo.		Indigo.	.000,158
		Blue.	Blue.	Blue.	Blue.	§§ 9, 6.	Blue.	.000,166
Light blue.	Stone gray.				Gray blue.	§§ 9, 6.	Gray blue.	.000,215
Blue green.					Blue green.	§§ 9, 6.	Blue green.	.000.252
Emerald green.	Olive green.	Green.	Green.	Green.	Green.	§§ 9, 6.	Pale green.	.000,268
Yellow green.	Yellow green.						Yellow green.	.000,275
					Green yellow.	§§ 9, 6.	Yellow green.	.000,282
Green yellow.						Jet of continually decreasing intensity of flow from the top to the bottom of this series of colors.	Green yellow.	.000,287
Light orange.	Bronze yellow.	Yellow.	Yellow.		Straw yellow.	§§ 9, 6.	Yellow.	.000,294
	Orange.						Orange.	.000,332
Dark orange.		Brown.			Muddy brown.	§ 9.	Brown orange.	.000,364
Pale scarlet.							Light Carmine.	.000,374
Pale purple.		Purple.	Purple.	Pale crimson. (Uncertain.)	Pale purple.	§ 9.	Purple.	.000,413
					Pale violet purple.	§ 9.	Violet Purple.	.000,421
					Pale violet.	§ 9.	Violet.	.000,433
		Blue.	Blue.		Pale indigo.	§ 9.	Indigo.	.000,455
							Dark blue.	.000,474
					Faint green.	§9, jet nearly shut off.	Green blue.	.000.499
		Green.					Green.	.000,550
					Faint yellow green.	Long tube, § 17.	Yellow green.	.000,564
		Yellow.			Beyond recognition.		Dull yellow.	.000,575

Kiessling's results need some explanation. His first series of colors is produced by compressing surcharged, suitably dusted air, and then allowing it to expand suddenly to atmospheric pressure. The experiment being made in a glass sphere illuminated by sunlight, the colors are observed in the axis of illumination. The succeeding series are obtained by aid of an exhaust pump. They are less easily located relatively to Newton's scale (see end of table) than Kiessling's first series. Indeed, I am at a loss to know, from his description, whether the whole of the four series (the colors gradually become fainter till they fade completely) can be obtained in a single exhaustion, or whether they are the results of successive independent exhaustions. I have inferred that the latter case holds and have so placed the colors. In any case one would expect them to become gradually more complex and mixed. The first series is intensely brilliant. Diffractions necessarily enter.

Aitken, who exhausts long tubes, gets a wider range of colors running (as I interpret them) from the first into the third order.

Von Helmholtz saw all his colors in the successive regions of an open steam jet, and reasonably inferred that the remote and cooler parts contain the larger particles. Aitken's jets play into tubes as stated above. §2.

With regard to my own work, most of the colors from the browns of the first order to the purples of the second can be produced in the tube, figure 3, by simply decreasing the pressure under which the steam issues from about 80 cm. of mercury to zero. But all the colors are dull, the initial browns muddy and the final purples so faint as to be hardly perceptible. However, to obtain the white, yellow, orange-brown, and brown-red of the first series in a state of great brilliancy, it is sufficient to let the steam jet suck the *flame of sulphur* into one of the air tubes *f*, figure 3, the others being closed. In this way particles of sulphur are volatilized, unchanged in an atmosphere of SO_2 and they reach the jet in a state of extreme comminution. Moreover, the *full* heat of the flame is sufficient to prevent all condensation (white), in spite of the sulphur nuclei; but by gently withdrawing the flame, the cloudy condensation gradually becomes incipient (yellow) and eventually more marked (brown). Hence these water globules must be of an extreme order of minuteness, their size increasing actually from zero (vapor) to

the dimension corresponding to brown. When the flame is withdrawn the field at first becomes opaque, and then runs through the blues and subsequent colors of the schedule, a result also agreeing with the inference stated.

Similar effects, but less striking, may be obtained with a Bunsen burner.

To obtain all the colors (except the initial white and light yellow, perhaps) brilliantly, and with a special view to minute differences of tint, the apparatus figure 4 or 6 must be used. This is the way in which the table was constructed.

In addition to the data of the table, the pressure in the steam box, which under fixed conditions gives a definite color, might have been added; but this is rather beyond the purposes of the present work.

11. *Possibility of Interference.* — If now the two parts of the above table be compared, there can be no doubt that the succession of the colors of cloudy condensation is identical with the corresponding succession in Newton's rings, of the first and second order,* seen by transmitted light under normal incidence. §12. Thus it is worth inquiring whether this is more than a coincidence, or whether small water globules, all of the same size, color *normally* transmitted white light, like thin plates.

For a given homogeneous color, if I be the intensity of the incident light and k the reflection coefficient (.04—.05 for the given conditions), then after a single transmission the interference maxima and minima are $(1-k)^2(1+k^2)I$ and $(1-k)^2(1-k^2)I$; that is they differ only very slightly and the rings are scarcely perceptible. But if there be an indefinite number of particles, all of the same size available, then this process is indefinitely repeated in such a way that while the light already colored is not extinguished, the admixed white light becomes continually more fully colored. Thus after a sufficient number of transmissions, the emergent ray may show intense color.

Hence, wherever particles are in great abundance, full and deep colors may be looked for; and when particles are not present in great quantity, the colors are necessarily pale, dependent as their intensity is on the *frequency* of transmission. Clearly when the jet is nearly shut off, the mixture is more di-

* The colors fade at the beginning of the third order.

ute then for intense jets, while in both cases the vapors fill the color tube by expansion.

12. *The opaque field.*— Between brown and dark violet in the colors of the first order there is an opaque region. Sometimes the brown takes on a reddish hue and sometimes the violet appears purplish. But the opaque region of indefinable color (§17) either in long or short color tubes remains. Indeed it is a characteristic landmark in the color territory. Being absent in exhaustions, it is clearly to be referred to the jet, and is to be interpreted in accordance with the researches of Osborne Reynolds,* on the flow of liquids in pipes. Reynolds found that a colored jet passing through clear water remains uniformly filamentary, so long as a certain critical velocity of efflux was not exceeded. After this the jet breaks up suddenly and violently into eddies. In the present experiments this critical velocity coincides for steam with the opaque field of the color tube; or, in other words, at this rate of efflux the stability of the jet breaks down. Here, therefore, one would scarcely expect to find particles of the same size throughout.

But this reason is not quite adequate; for if the intensity of the jet be still further increased, magnificent browns and oranges appear. To account for opacity it is thus essential to inquire into the relation of color to size of particle, at the point where color is extinguished.

In the annexed diagram, Fig. 7, I have laid off Newton's colors as abscissæ, by simply platting off equal distances for each step of color. The ordinates are the corresponding diameters of the particles as given in the above table, and referred to air.† Thus it is seen at a glance that whereas between white and brown, and between violet and green, the colors change slowly and uniformly for relatively rapid increments of diameter, quite the converse of this holds good for the intermediate space between brown and violet. These hues would be very sensitive as a criterion for variations of the size of particles. Furthermore a mixture of these colors is virtually black to the given conditions of transmitted light. Hence if a stable but rapidly

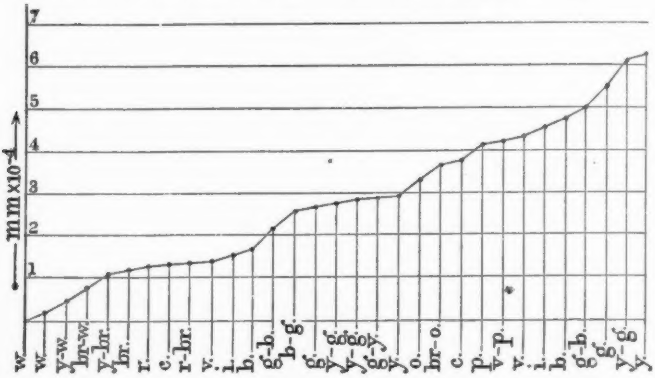
* Reynolds: Phil. Trans., London, III., p. 935, 1883, cf. p. 957.

† The water particles being smaller than this in the inverse ratios of the indices of refraction. Nothing would be gained by platting cubes of diameters.

flowing steam jet topples out of equilibrium just at this region of colors, an opaque field would seem to be inevitable.

A similar explanation may be offered to the straw color of the

Fig. 7.—Diagram showing the Colors of Newton's Interferences by Normally Transmitted White Light, as Dependent on the Thickness of the Plate in Ten-Thousandths of a Millimeter.



yellow following the blue-greens; but here the insufficiency of ventilation is already a matter of consequence.

13. *Color tube by reflected light.* — If the jet which has been made opaque by dust or otherwise, and which in the free air appears blue, (say) by transmitted light, be projected against a dark background, the complementary purple appears distinctly. Similarly in the color tube, if any part of the field be dark, this tends to take on the complementary hue of the field proper. Phenomena such as these are not simple, for they may result from physiological contrast and be merely subjective, or they may be truly physical.

Hence I made a special study by aid of the apparatus figure 5, where the only light which can reach the eye (after proper screening) is that reflected by the steam corpuscles themselves. Looking into a tube of this kind, when no steam is admitted, the sides and opaque top *a* are faintly seen. Again, when pure steam, either without air or with filtered air, passes, the colorless clearness remains and the presence of steam reveals itself to the eye only by the shifting and dancing of the interior. When, however, dusty air is admitted, the column is at once opaque, of a pale neutral white color, and, so far as I could see,

it makes no difference what the color of the transmitted light may be, or how the tube is manipulated.

Now this is precisely what one would expect, since the case is nearly the converse of § 11. For a single particle and homogeneous light, the beams which interfere are $k\,I$ and $k\,I\,(1-k)^2$; *i. e.*, they are weak but nearly equal in intensity, and the interference is very perfect and brilliant. But it is not capable of indefinite repetition, for after each interference the direction of the ray is reversed. Hence such light, if at any depth, is either entrapped in the color tube or it can only emerge from the tube (no matter whether the front or rear end be considered) by being transmitted *through* the particles as in § 11. Light, however, which has already been colored by reflective interference and is now also compelled to interfere by repeated transmission, can only have retained the colors simultaneously present in both complementary colors. Hence it is virtually extinguished, and all one would expect to see is a dull white effect of diffuse light. In the same way light colored by repeated transmission could not survive reflective interference.

Finally the only case in which colors by reflection are anticipative is the one presented at the beginning of this paragraph,—the case where the free mantle of the jet is looked at against a dark background; for inasmuch as the interference by reflection is under like conditions much more intense than interference by transmission, the former, up to a given normal depth, might well prevail over the latter and hence be appreciable.

14. *Size of corpuscles and summary.* — In so far as the views just presented are correct, the size of the prevailing particles is at once given in terms of the colors of the column seen by transmitted light. It is merely necessary to divide the data of the table in § 10 by the refractive index of water (practically to multiply those numbers by three fourths) to obtain the diameters of the particles producing a given color. Thus it appears that if the prevailing diameter be (say) 0.000,004 centimeters, the first of the visible colors, the yellow of the first order looms into view. If (at the present stage of completion of my apparatus, § 9) the prevailing diameter be 0.000,040 centimeters, the last of the visible colors, the faint green at the end of the second order will appear. Between these extremes something over fifteen different steps of diameter are recognizable.

It is interesting to compare this order of values with the data obtained by v. Helmholtz (l. c.). Utilizing the equation due to Lord Kelvin,* according to which, at a fixed pressure and temperature, the excess of vapor tension at a convex spherical surface is directly proportional to the surface tension and inversely to the radius of curvature, v. Helmholtz found by direct experiment that diameters between 0.000,015 and 0.000,026 centimeters must have been encountered in his work. This is very near the mean of the values found above from interference data. It corresponds to the field between blue of the first order and purple of the second order, and this is the most intense and persistent part of the field.

To recapitulate. If the colors of cloudy condensation seen under normal incidence (in tubes) be regarded as cases of Newton's interferences by transmission, the order of succession, the occurrence of intensity and of faintness of color, the position of opacity, the absence or partial absence of color by reflected light, and the mean size of the active particles is well represented. This does not yet amount to a proof of the proposition as against diffraction ; † but it is certain that the increase or decrease of the size of particles has been correctly interpreted in terms of color qualitatively at least if not quantitatively, and that the absolute sizes derived cannot be far wrong. Bearing this in mind the color scale may be safely accepted. I shall probably recur to the matter more rigorously at some other time, and whatever correction be necessary can then easily be applied.

15. *The condensation problem.*—With these views laid down, the remarks of earlier paragraphs (§§ 1, 9), may be resumed. In the differential apparatus, Fig. 6, let the two tubes A and A' be connected in series as shown in the annexed diagram, Fig. 8. Here the jet of A' is removed, and the vapor discharged from A is conveyed by a sufficient length of tubing D, of the same diameter into A'.

Then the progress of the condensation, *i. e.*, the difference in the size of water globules in A and A', is at once given by the difference of color of the two tubes.

The time taken by the vapor in making this passage, or the

* Sir William Thomson: Proc. Roy. Soc. Ed., 1880.

† For in both cases the superposed maxima amd minima must for any homogeneous color lie at distances apart proportional to the wave length.

velocity of the current, can be accurately determined as follows: after a permanent colored field has been established, let the flame of a Bunsen burner be quickly moved across the air hole *C.* The effect of this is to superheat the steam issuing from the jet, and therefore a momentary flash of bright light is seen in *A*, followed at once by an opaque field due to the (cold) dust in

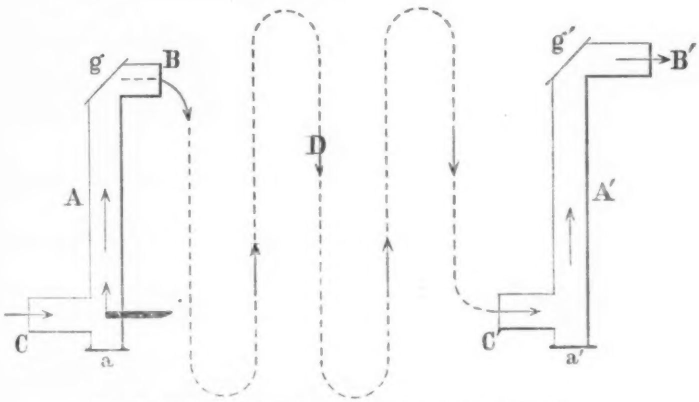


Fig. 8.—Differential Color Tubes. Arranged in Series.

the wake of the flame. This flash travels like a clear bubble through the whole length of tubing *D*, and is eventually seen at *A′* quite as brilliantly as at *A*. It is therefore only necessary to note the time between the transit of the Bunsen flame across *C*, and the flash in *A′* in order, with the known length of tubing *D*, to determine the rate of flow.

Inasmuch as this is a problem into which I am entering at some length, I will here only cite a random example: —

Length of tubing *A D A′* (with allowances for ends), 300 cm.

Time between flashes, 1.2 seconds.

Color of *A*. Green, yellow, blue, opaque.

Color of *A′*. Green, yellow, blue, opaque.

In other words under these conditions there is *no perceptible condensation* (*i. e.*, enlargement or decrease of the water globules) *in six-fifths seconds.* It follows from this result that a very considerable length of tubing, *D*, is required in these experiments. One difficulty is to be guarded against: the train *D* introduces an appreciable resistance to flow. Hence the ven-

tilation is apt to fall below the necessary value, whereupon the colors in A, and particularly in A' become dull. I think this can be counteracted by attaching an auxiliary vertical tube with an especial ventilating jet at B'. In this way the experiment proper is left without interference.

Quite a number of other questions, chiefly in reference to the flow of gases in tubes, and to jets, suggest themselves for solution here, into which I will not enter. Temperatures at any part of the system may be measured, thermoelectrically at least, and within certain limits they may be varied. Concentrated sunlight, for instance, can be made to actually permeate the substance of the current. Again, the discharges of two independent different colored tubes A, may be mixed in the train D, and caused to react on each other, while the effect is observed in A', etc.

The other two factors which enter prominently into the present work, are the amount of adiabatic cooling at the jet (which is, perhaps, open to computation and also to thermoelectric measurement, and which may be varied by connecting the color tubes in multiple arc as shown in Fig. 6, and putting a stopcock in one of the tubes, *b*, Fig. 1), and the temperature of the medium into which the jet discharges. To study the latter case (supposing the color tubes connected in multiple arc, Fig. 6), C is to be joined with a train of tubing supplying air at any temperature and suitably dusted.

Results so obtained, in addition to their meteorologic bearing, have a general physical significance as well. As to the reasons * why an intense or intensely supersaturated jet is (caet. par.) shattered into smaller fragments than a gentler efflux will be briefly indicated in §19. The three experimental facts on which I am justified in putting stress are (1): the occurrence of a marked degree of supersaturation; (2) the color producing particles are an almost *instantaneous* growth; (3) after being produced they retain their individuality as to size indefinitely (relatively speaking and proper conditions presupposed). These, I think, will adequately account for the sudden action and the equally sudden and definite cessation of the drop producing tendency exhibited in the above experiments.

* The ingenious and picturesque view of the case taken by Mr. Aitken will be found throughout his paper, but particularly on page 423. Cf. §2 above.

16. *Dust and flames.* — Utilizing the apparatus above, I made a great number of special experiments, into which, however, I can only enter here in a cursory way.

If, when the apparatus, figure 3, is in action and the field of the usual gray-blue color, any dust be set free in the neighborhood of the air tube *f*, the field at once becomes opaque. What is meant by dust here is not so much the noisome article of household economy nor smoke *necessarily*. Dust in the present sense is preferably far more comminuted material (§ 19), such for instance as arises out of a clear perfect Bunsen flame, or from glowing (not smoking) charcoal (Japanese punk), or from the sodium tinge in the outer mantel of an alcohol burner, or from a sulphur flame (Kiessling), or from the surface of an almost tensionless liquid like ordinary concentrated sulphuric acid (v. Helmholtz), etc. Take the apparatus (Fig. 3) with all the air-holes open, and let a small clear Bunsen flame be placed on the table above the level of *f*. No effect is produced on the color of the field; but if the same burner is put on the floor in the free air, much over a meter below *f* and even laterally from it, the densely opaque field soon appears and persists for some time even after the burner is again placed above the table. This conveys some notion of the sensitiveness of this apparatus, and the dusty exhalation of the little flame.

Indeed, if the air tube *C* of the apparatus Fig. 4 be prolonged as far as the ceiling of the room, it will catch the fountain of dust issuing from a small Bunsen flame on the floor at a distance of twelve feet below the mouth of the tube. The field, all but colorless before (faint blue, second order), becomes densely opaque.

As it seems to me, the underlying cause of this striking activity is not merely the traplike action by which the flame collects atmospheric dust, but rather the additional fact that many of the great chunks of such material (metaphorically speaking) are heated to the point of disintegration and volatilization. Thus a whole cloud of nuclei comes into being where a single one existed before; and thus one might expect the hot flames like burning oxyhydrogen (Coulier) to be more active than cold flames like alcohol, since a scattering of planets into nebulæ is the work to be done. R. v. Helmholtz,* who is among the chief

* Von Helmholtz: l. c. p. 7, 8.

students of this particular subject, calls attention to Giese's * important discovery of the diffusion of ions out of the flame proper, and their continued activity † at some distance from it on all sides. Von Helmholtz regards it not improbable that many of these ions are neutralized in the steam jet and there by a kind of molecular percussion induce cloudy condensation — an exceedingly beautiful hypothesis which he is loth to abandon, although throughout his paper his stronger judgment seems constantly to urge him in the direction of more humble references to atmospheric dust. Von Helmholtz does not make a clear case; but the question is an open one and a decision will be difficult; for the experiments to be worth anything must be faultless.

17. *Promiscuous Experiments.* — If a compressed gas (oxygen) be admitted into the color tube by a collateral jet, the colored field becomes clearer. Admitted with steam into the steam box, the gas pressure replaces the steam pressure, so that without further increasing the flow of steam, the colors can be made to pass from the second to the first order, through brown, orange, yellow, almost to white. Thus by using very little steam (the jet alone showing greenish of the second order), and increasing the gas pressure, I have chased the colors through "opaque," without losing a transparent field. In this way I sometimes detected a reddish hue on passing from dark violet to brown; but as a rule the transition is smoky and peculiarly indefinable. The chief experimental advantage of this arrangement is the occurrence of a jet of a decidedly *lower temperature* than the pure steam jet. The quantitative value of such a comparison is obvious. If a jet with but one hole, 0.15 cm. in diameter, be compared with a jet containing a number of holes of the same size (differential apparatus, other things equal) and if pressure is continually increased, the latter will reach the opaque field sooner than the single holed jet; after this the action is reversed, so that the single jet reaches the initial orange sooner than the other, if the other passes beyond opaque at all. When there are more holes than one, the faint colors of the second order are more obvious (as might be supposed), and a peculiar

* Giese: Wied. Ann., xvii., pp. 236, 519, 1889.

† Recalling the morerecent researches of Arrhenius (Wied. Ann., xlii., p. 18, 1891) that a sodium flame is a better electrolytric conductor in virtue of the solution of sodium, there is some doubt in my mind as to the sufficiency of Giese's result for v. Helmholtz's purposes.

feature is the persistence of intense emerald greens. If the jet contains as many as five holes, the colors become dull. The same dulness is observed for a single jet when the air hole, *C*, Fig. 9, is partially stopped up. Cf. §§ 6, 7. These experiments have an important bearing on the theory of the condensation process.

Corresponding experiments were made with long tubes, using a single jet. Thus I found by decreasing the length gradually from 220 cm. to 150 cm., that for all jet intensities the field remains opaque. On further decreasing the length the colors gradually appear, but even at 110 cm. they are dull. Tubes of this kind are serviceable for bringing out the green and green yellow near the beginning of the third order of colors. § 10.

The temperature of the walls of the color tube does not seem to produce a marked result. Thus I put a wide helix of lead pipe, through which cold water was flowing, into the color tube, figure 3, but obtained no noteworthy effect beyond a certain loss by condensation. The differential apparatus was not tested in this way.

Fumes of naphthalene, camphor, alcohol, ether, turpentine, etc., when pure, showed no dustlike action. A space filled with naphthalene fumes is rather cleansed of active dust, and I suspect that this action is connected with the formation of little crystals. For purposes like the present, therefore, air in which naphthalene fumes have condensed may possibly be used for filtered air, with advantage.

Sulphur chloride (which is decomposed by water liberating sulphur) acts as actively as sulphuric acid.

The action of sulphur itself is more interesting. The effect of the flame has already been described in § 10. Let the flame be extinguished, and the sulphur cooled down to the point of incipient freezing, *i. e.*, until the appearance of the surface changes from glossy brown to opaque yellow. On bringing this sample near the air-hole *C* of the differential apparatus, the field at once clouds over, even if the influx pipe is above two meters long. As the sulphur solidifies further, the series of colors is slowly run through. This experiment might serve to illustrate the difference of vapor tension at the solid and liquid surfaces of the same substance, at the same temperature (Ramsay and Young, Fischer). Furthermore, a stick of sulphur

softened in a burner suggests itself as a convenient method of showing the succession of colors described in the above table.

As compared with moist air, dry air must be the better dust carrier, for the particles are not clogged with moisture. The surmise is reasonable, moreover, that a dried particle may be peculiarly effective. Experiments made with air passed over chloride of calcium and phosphorus pentoxide negatived this These bodies are neutral. Hence, in the case of sulphuric acid, it is actually the vapor which is the active principle, unless, indeed, an air ammonic sulphate be produced.

By far the best dust producer which I have found is a freshly-cut surface of phosphorus at ordinary temperatures; and its activity persists for a long time (certainly many hours) after cutting. The fingers rubbed with such phosphorus are also active. If air aspirated over a piece of phosphorus as big as a pea be mixed with different quantities of room air (coalescence of the conveyance tubes) persistent colors correspond to each degree of mixture, and they may thus be produced at the minimum (all but vanishing) steam pressure. The active agent here is the invisible dust of oxidation; for ozone, according to Mascart, is inactive.

It is important to notice that the yellows of the first order are not affected by dust, — another argument in favor of their origin from exceptionally small water globules.

To the effect of the temperature of the air into which the steam is discharged, I shall devote a special paper. Suffice it here to say that below 9° C. the field is opaque at all pressures. As temperature increases, the steam pressure at which the blue of the first order passes into opaque increases from zero rapidly to a maximum; whereas the pressure at which the yellows of the first order pass into opaque decreases from an enormous value, at even a more rapid numerical rate than before, to a minimum, in such a way that, for normally dusty air, the said maximum and minimum coincide at about 40 cm. of mercury (pressure) and 35° C. Here, therefore, the blue passes into the yellow without an opaque demarcation. Beyond this (air temperature increasing) the colors which have gradually grown fainter vanish. The tube is clear and colorless, showing that the supersaturation at the jet is *nil*.

18. *Electric excitation.* — Von Helmholtz, in his paper on the electrification of jets, leaves the question as to the cause of the phenomenon open, indicating that either the solid matter thrown off from the highly charged point, or the diffused ions may be the active agent. His attitude as regards these two hypotheses (both of which he examines elaborately) has virtually been given in § 16, *mut. mut.*

Mr. Aitken throws himself vigorously into his own modification of Bidwell's hypothesis, as already stated in §2. Aitken supposes that the comminution of drops, necessary to produce an opaque jet, is here maintained in virtue of their electric charges, since the repulsions thus set in action prevent a coalescence of drops. It is noteworthy that no effect is produced, unless the exciting electric charge escapes from a point, so that high potential is essential.

Now, I was somewhat surprised to find an observer, of Mr. Aitken's acuteness, entirely ignoring the misgivings of his predecessor, and particularly so, since the recent experiments of Crookes on electric evaporation might well have emphasized the need of such precautions as v. Helmholtz took. For if there be any solid matter escaping from an electric point, the whole argument in favor of a truly electric condensation in steam jets is vitiated.

I made this test experiment: In the apparatus, figure 3, the jet *j* is in the inside of a practically closed conductor. Hence, it will not be acted on by an electric field on the outside of the apparatus. To give further assurance that this was the case, the (only) open air tube *f* was prolonged even two meters or more. I then caused the spark of a Ruhmkorff coil to pass across a gap, made by two copper needles lying in the same line, with their sharp ends about a centimeter apart. Whenever this arrangement was brought near the open end of *f* (supposing the apparatus in action) the blue-gray field became opaque. Something, therefore, passed between the points of the spark gap, whether metallic dust or other material, which was simply sucked into *f* by the draught, and which affected the jet precisely as a bit of glowing punk would have done. I then adjusted the inductor so that sparks passed at the coil only, and none were visible at the portable spark gap. On again bringing the latter near the open end of *f*, the darkening of the field was

evident enough, though not so intense as in the previous case. Even a silent discharge is therefore active.

The same results were obtained with a clean platinum spark gap.

This experiment may be made more beautifully with the differential apparatus, figure 6. Connect the air hole C of the color tube AA with an equally wide tube running vertically upwards about two meters or more away from the jet. Then fix the spark gap in front of the open (upper) end of this auxiliary tube. The key of the induction coil may be held in the hand. Let the initial field of both AA and A^1A^1 be faint violet (say) of the second order. Then as long as sparks pass across the gap, the field of AA turns deep blue or opaque ; or, in general, changes in color in the direction from the second to the first order, as the spark gap widens. Indeed, it is possible to obtain almost any of the colors with very little steam, quite permanently in this way, by simply regulating the spark lengths. The field of A^1A^1 does not change in color. Silent discharges (*i. e.*, mere alternate chargings without sparks) act definitely but with less intensity.

Hence I conclude that the electrification of a jet by means of a point is at most a complex phenomenon. Although the effect of an electrical field on water jets is beyond question, no unassailable experiment has yet been made to exhibit the occurrence of a similar truly electric effect in the case of the cloudy condensation in steam jets. And since only that part of the efflux which lies near the nozzle is influenced, the clear case in favor of the promotion of cloudy condensation by electrification *as such* is yet to be made. My own belief is that the evidence will not be forthcoming for reasons set forth in § 19.

I may note, in passing, that in the present experiments (spark lengths increased) augmentation of the number of dust particles produces the same result as did the increased supersaturation in the earlier work. A similar test may be made by holding the mouth of a sulphuric acid bottle (or hot sulphur, etc.) near the open end of the air tube C and then gradually withdrawing the bottle. The field will be found to run through the gamut of colors from opaque to the second order. I accentuate these observations because of their theoretical importance (§ 19), remembering that the field at the outset (before adding dust) was pale violet

or indigo of the second order and the jet nearly shut off. Thus with little supersaturation the appearance of intense colors and smaller particles is somewhat startling. In no case have I in this way been able to produce the yellows and browns of the first order beyond the opaque, except by using the flame of sulphur. Here, however, heat enters as a factor.

One would expect the particles of the steam jet to be electrically charged, for the reasons given in Faraday's classical researches. I thought it worth while to test the case for a metallic jet, however, as follows: The brass nozzle was electrically connected with the earth and the charge of the jet collected by allowing it to play against an insulated metallic brush communicating with a Mascart electrometer. Producing a blue-gray field in one tube of the differential apparatus, I found that a maximum voltage of + 10 was easily collected at the free jet of the other. This maximum increases and decreases markedly with the intensity of the jet. The positive charge is also decreased whenever a drop squirts from the nozzle. No difference between the maximum voltage of an opaque (sulphur) and an ordinary jet (caet. par.) could be detected, but I do not consider my experiments final.

19. *Conclusion.** — In the above pages I have endeavored to show that a solution of the condensation problem may be regarded as hopeful for a certain class of water globules ranging in size from somewhat less than the sodium wave length of light to about one tenth of this magnitude; in other words the growth of the globule, from the latter to the former of these dimensions, can be controlled and studied. For particles smaller and particles larger than the limits given, the method (at its present stage of development) fails. Very frequently, however, laws carefully scrutinized within a given range suggest the behavior within the next range (as I will presently show); or else the method of the one case leads to modifications of method for the other. If 0.000,03 cm. be taken as the limit of microscopic separation, then particles whose size is just exceeding the limit of interference measurement (0.000,040 cm., being the green of the second order and the above apparatus) are just becoming

* Reference to Hertz, von Bezold, and other allied researches (see Prof. Abbe's collection in Smithsonian Misc. Coll., Washington, 1891), will be duly made in my subsequent papers. The present paragraph is a mere outline.

microscopically measurable. The problem thus begins to deal with something decidedly more tangible.

In conclusion I have to advert more definitely to the point of view taken in § 1, trusting that the intervening paragraphs have shown the need of a more detailed knowledge of the true isothermals for the change of state gas-liquid. By the term true isothermals I wish to indicate that the behavior of a *free* fluid is meant; *i. e.*, one in which there is neither unclear nor surface condensation, or virtually a dust-free fluid in a vessel of extensible and absolutely neutral walls. No such vessel exists; hence all the experiments must be of an instantaneous kind so that the results may be noted before the vessel has time to distort them.

Necessarily the isothermals * for this ideal case must be of fundamental importance. Their chief feature may be already inferred. Near the region of changes of state, all are characterized by a pronounced volume-lag, in virtue of which at a given temperature, it takes a higher pressure to change the pure dry vapor back to the liquid state than the pressure at which the liquid just wholly vaporized.

To take a concrete example, let the volume of a given mass of water at 100° C., be allowed to expand isothermally until the whole bulk is quite converted into dry steam. The pressure so obtained is the vapor tension of water at 100°, *i. e.*, one atmosphere. Then if the vapor be dust free and contained in the ideal vessel, no condensation can take place on again decreasing the volume isothermally, at 100, until the pressure has reached a remarkably high value. For by hypothesis the only nuclei present must be a collection of water-vapor molecules and must therefore be of molecular dimensions. Hence the condensation pressure must reach the vapor tension at 100° C., at an extremely convex surface. It is interesting to estimate in a rough way about how large this pressure will be. The distance between the centres of two adjacent water molecules is probably greater than 0.000,000,01 cm., and probably less than 0.000,000,1 cm. At 100° the density of dry steam is (say) 0.0006 and of water 0.96. Finally the surface tension of water in contact with

* It is customary to represent the volume of a given mass of fluid as depending on pressure and on temperature conjointly, and convenient for the time being to consider either temperature constant (isothermals) or else to consider pressure constant (isopiestics).

air is at 20°, 80 dynes per linear centimeter. At 100° and in contact with steam the surface tension will be considerably less than this, but the datum given may be temporarily admitted. Hence the *excess* of vapor tension at a convex (molecular) surface of the diameter 0.000,000,01 cm. is 20 atmospheres; and the corresponding excess at a convex (molecular) surface of the diameter 0.000,000,1 cm. is two atmospheres. One should, therefore, be prepared to exert about 10 atmospheres, in order to condense * pure dust-free dry steam at 100° C., in an ideal vessel of the kind in question. When the necessary pressure has been brought to bear the steam would collapse as a whole, supposing, of course, that temperature never exceeds 100° C.

If instead of pure steam, a mixture of dust-free air and dry steam be given, the phenomenon, in its general features, would retain the character just sketched; for here also there is an absence of other than molecular nuclei. In one respect, however, a difference is manifest: when the condensation pressure is exceeded, the steam would not condense out of air, as a whole, for the case is now virtually one of solution. In other words, the interaction of air and steam is brought into play and successively increasing pressures correspond to successive states of chemical equilibrium. Cf. § 1, for the analogous case of liquid solutions.

This condition of things changes enormously when dust is introduced either into pure steam or into the air mixture If the diameters of the dusty particles were only one hundred times as large as the smaller estimate of molecular diameter (10^{-8} cm.), *i. e.*, if the dust particles were only one millionth of a centimeter thick, they would already reduce the condensation pressure to

* I think the above experiment actually feasible, not isothermally, of course, but adiabatically. If it is performed it must lead to a superior limit of the absolute size of the molecule, either of liquid water or of any other liquid similarly treated. From this point of view such a research seems to me to be of unusual interest, and it may well be included as a part of my problem.

Again in the case of freezing, the volume lag can actually be measured, as I showed in the American Journal of Science, xlii., p. 125, 1891. In terms of van't Hoff's analogy, volume lag expressed in atmospheres would be the increase of osmotic pressure at a convex molecular surface. Hence a reason is given by Thomson's equation for the undercooling of liquids; and the absolute molecular dimension of a solid molecule is defined in terms of known variables and the surface tension of the solid, a quantity which has already been recognized in Auerbach's researches on hardness (Wied. Ann., xliii., p. 61, 1891. Cf. p. 94). Thus a scheme for the absolute measurement of solid and liquid molecular dimensions is suggested.

about 15 cm. of mercury. Dust particles, one thousand molecular diameters in thickness, would reduce it to about one or two centimeters.* Moreover, the smaller of these particles (taking their density at about 2) would fall through air at the rate of less than 0.000,0006 cm. per second, and would be an integrant part of the air. Indeed, even the larger dust particles (0.000,010 cm. thick), falling at the rate of .000,06 cm. per second, could scarcely be separated except by filtration. The marked activity of very fine dust (§ 16) is thus accounted for, and the term "dust-free" air is to be used with caution.

Instead of keeping temperature constant and studying volume in its dependence on pressure, the latter variable may be kept constant, and volume considered in its dependence on temperature.

The inferences are necessarily identical, remembering that an increase of pressure corresponds in its effects on volume to a decrease of temperature. Practically, the methods are more complicated. The only way of producing the condensation in question is some form of adiabatic cooling, so that pressure and temperature are varied simultaneously. But the effects may nevertheless be treated with reference to the elementary cases specified.

It is now in place to endeavor to apply these preliminaries to the actual case of the steam jet. I have worked this out at length; but without more detailed experiments, I am loth to communicate it. A brief mention of the method pursued will here suffice. It consists essentially in a comparison of the vapor tension at the convex surface of the globule of water, and that of the supersaturated steam immediately surrounding it.

If while the particle is growing there were no change of its temperature, nor the pressure of the surrounding medium, then the vapor tension at the convex surface would continually decrease as the size increases. Actually, however, the particle is being heated by the condensation of the water which promotes its growth. The case is therefore not excluded, that a particle increasing in size will show increasing vapor tension at its convex surface, more markedly so in proportion as the number of

* These numbers would be smaller if the temperature correction for surface ten on were applied. These pressure differences or their equivalent temperatures are to be expected in steam jets.

active dust particles per unit of volume is greater. But this is rather beyond the limits set for the present considerations. It is necessary, however, that at the outset the tension at the surface of the globule be virtually less than the tension of the supersaturated medium, supposing the phenomenon to be looked at by the above isothermal method.

Now the full growth of the particle is attained in an incredibly short space of time, probably in very much less than the hundredth of a second; whereas when fully grown it persists under the given conditions, without change of size, for an incredibly long time, certainly many seconds. §15. Hence the medium around the particle, losing volume in virtue of condensation, expands adiabatically.

Thus I derive two descriptive equations or curves, — one expressing the tension at the surface of the particle in terms of its size, and the other the tension of the immediate neighborhood referred to the same size. If these curves did not intersect, the particle would continue to grow forever. But if they do intersect, then there is a point at which the initially smaller vapor tension at the surface of the particle is equal to that of the immediate medium, and beyond which (size increasing) the vapor tension at the surface would exceed that of the medium. This point, therefore, fixes the dimension of the particle stable under the conditions for which the curves were drawn.

These principles contain an easy working hypothesis for the construction of a homogeneous theory of the above condensation phenomena. It therefore scarcely seems worth while to offer further comments, until the whole question has been subjected quantitatively to a minute investigation.

Since writing the above I have looked minutely at the thermal topography of the steam jet, studying the thermal distribution at all points throughout the length and breadth of the jet for different temperatures of the surrounding air and for different actuating steam pressures. The feature of these results (which are an essential step in my quantitative work) is the occurrence of temperatures below the normal boiling point even near the nozzle of the jet, and, therefore of a marked degree of supersaturation as compared with the steam before efflux.

I have also constructed a dust counter based on the *pressure* at which the blues of the first order pass into opaque. § 17. This instrument may, therefore, be said to be independent of color estimates or discriminations. The pressure in question is a function of both temperature and dustiness, and I have worked out the temperature relations leaving dustiness as the remaining parameter.

PHYSICAL LABORATORY, U. S. WEATHER BUREAU.